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J. PHILOSOPHY

VOL. 81 1984 ii 8—12.

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THE JOURNAL OF PHILOSOPHY

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THE JOURNAL OF PHILOSOPHY

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All communications to the editors, manuscripts, advertisements, subscriptions, and changes of address should be sent to the Managing Editor. Annual subscription price (12 numbers), to individuals, $15.00; to institutions, $20.00; to students and the retired and the unemployed, $12.00; current issues, $2.00; special issues, $3.00. All back volumes and separate issues are available back to 1904. Published monthly as of January 1977; printed by Lancaster Press, Inc., Lancaster, Pennsylvania.

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ISSN 0022-362X

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THE JOURNAL OF PHILOSOPHY
VOLUME LXXXI, NO. 8, AUGUST 1984

WHAT IS A THEORY OF TRUTH?*

ALFRED TARSKI'S theory of truth and its successors enjoy a perplexing double status. On the one hand, they are mathematical theories characterized by a rich class of mathematical results. On the other hand, they are commonly believed by philosophers to provide analyses of the nature of truth and, hence, to be philosophically significant. With this broader significance comes a kind of controversy not normally associated with mathematical theorems. No one disputes the correctness of Tarski's formal results. In that sense, there is no doubt that his theory is true. However, there is considerable doubt about whether, or in what sense, it is a theory of truth.

One main reason for this uncertainty is the difficulty of determining what a theory of truth ought to be. Generally, theories of truth have tried to do one or the other of three main things:

(i) to give the meaning of natural-language truth predicates;
(ii) to replace such predicates with substitutes, often formally defined, designed to further some reductionist program; or
(iii) to use some antecedently understood notion of truth for broader philosophical purposes, such as explicating the notion of meaning or defending one or another metaphysical view.

In order to do the first of these things, a theory must analyze the content of paradigmatic examples in which what is said to be true is a proposition, rather than a sentence or utterance.

(1) a. The proposition that the earth moves is true.
b. Church's theorem is true.
c. Everything he said is true.

*This paper was presented in the spring of 1983 at Yale and Dartmouth, where I profited from useful discussion. Thanks are also due to Paul Benacerraf, Axel Buhler, John Burgess, Gilbert Harman, David Lewis, and Walter Sinnott-Armstrong for reading and commenting on an earlier draft.

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There are theories that try, in my opinion unsuccessfully, to do just this.[1] Tarski's theory, which restricts itself to cases in which truth is predicated of sentences of certain formal languages, is not one of them. Thus, Tarski cannot be seen as even attempting to give the meaning of natural-language truth predicates.

Nor can he be seen as attempting to use the notion of truth for broad philosophical purposes. In order to do that, one must regard truth as essentially unproblematic and philosophically productive. For Tarski, truth itself is what has to be legitimated. Once it is, it turns out to be useful for certain primarily technical purposes, but useless for ambitious philosophical programs. For example, Tarski recognized that his notion of truth could not be used to give the meanings of logical constants (or, I suspect, anything else).[2] He also thought of it as epistemologically and metaphysically neutral. Thus, in "The Semantic Conception of Truth" he says:

> . . . the semantic definition of truth implies nothing regarding the conditions under which a sentence like . . .
>
> *snow is white*
>
> can be asserted. It implies only that, whenever we assert or reject this sentence, we must be ready to assert or reject the correlated sentence . . .
>
> *The sentence "snow is white" is true.*
>
> Thus, we may accept the semantical conception of truth without giving up any epistemological attitude we may ever have had; we may remain naive realists, critical realists or idealists, empiricists or metaphysicians—whatever we were before. The semantic conception is completely neutral toward all these issues (33/4).

It is helpful, in understanding this remark, to focus on something that the truth predicate is good for—namely, what W. V. Quine has called "semantic ascent."[3] The simplest example of this is provided by Tarski:

(2) a. Snow is white.
    b. The sentence 'Snow is white' is true.

Any speaker of English knows that these sentences are at least materially equivalent. Because of this, they can often be used to convey essentially the same information. To choose (2b) is to use a semantic statement to convey information that could have been

[1] Various versions of the "redundancy theory" fall into this category. Although these versions deal with "propositional" contexts like those in (1), they deny that 'true' is predicated of propositions, or anything else. Arguments against these approaches are given in my *Semantic Theories of Truth* (New York: Oxford, in preparation).

[2] "The Semantic Conception of Truth," sec. 15, in Leonard Linski, ed., *Semantics and the Philosophy of Language* (Urbana: U of Illinois Press, 1952).

[3] *The Philosophy of Logic* (Englewood Cliffs, N.J.: Prentice-Hall, 1970), pp. 10–13.

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conveyed nonmetalinguistically. To do this is to engage in semantic ascent.

The importance of semantic ascent is illustrated by cases like (3), in which we want to generalize.

(3) a. Snow is white → (Grass is blue → Snow is white)
b. The earth moves → (The sun is cold → The earth moves)
⋮

Each of these examples is something one could feel safe in asserting. However, if one wanted to get the effect of asserting all of them, one would have to quantify, replacing sentences with variables. In English such quantification is most naturally, though not inevitably, construed as first-order and objectual. Thus, if the variables are taken to range over sentences we need a metalinguistic truth predicate. Semantic ascent gives us

(4) For all sentences p, q (p is true → (q is true → p is true))[4]

That which is conveyed by (4) is closely related to that which is conveyed by (3). However, here the truth predicate is especially handy, since we don't have the alternative of asserting each member of (3).

Truth predicates can be used in the same way in more obviously philosophical cases. For example, consider:

(5) There is a duplicate of our sun in some remote region of space, but we will never find (sufficient) evidence that there is.

Someone who asserted this would, by contemporary standards, be counted a metaphysical realist—i.e. as being someone who thinks that what there is doesn't depend in any way on what we may rationally believe. Of course, one can be a realist without believing (5). One may think that what there is doesn't depend on us, while believing that there is no duplicate of our sun, or being uncertain whether there is, or while holding that evidence will someday be found to settle the matter.

What, then, distinguishes realism from anti-realism? One is tempted to answer that it is the belief that

(6) Either there is a duplicate of our sun in some remote region of space, but we will never find (sufficient) evidence that there is; or there is no duplicate of our sun in any remote region of space, but we will never find (sufficient) evidence that there isn't; or

[4] Or, equivalently, (4′):
(4′) For all sentences p, q $\ulcorner(p \rightarrow (q \rightarrow p))\urcorner$ is true.

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there is intelligent life elsewhere in the universe, but we will never find (sufficient) evidence that there is; or

⋮

But this is awkward. We ought to be able to state the realist's position without having to gesture toward an infinite list. Semantic ascent provides a convenient way of doing this. With the help of a truth predicate and quantification over sentences, we can characterize the realist as believing, and the anti-realist as denying:

(7) There is at least one sentence *s* such that *s* is true (in English), but we will never find (sufficient) evidence supporting *s*.

The relationship between (6) and (7) is like that between (2a) and (2b) and between (3) and (4). In each case, the semantic sentence may not say exactly the same thing as its nonsemantic counterpart; but if knowledge of English is assumed, the two can be used to convey essentially the same information.[5]

The utility of the truth predicate in stating this dispute has led some to believe that the dispute is about truth and, hence, that truth is a deeply metaphysical notion. However, there is no reason to suppose this. The realist and anti-realist may agree about truth; they may even accept something like Tarski's definition. Where they differ is in their conceptions of reality. Since statements about truth mirror direct statements about nonlinguistic reality, semantic ascent makes the truth predicate a convenient vehicle for expressing competing metaphysical views. But a convenient vehicle is all it is. As Tarski puts it, the notion of truth is completely neutral toward all these issues.

The upshot of this is that Tarski's definition of truth is neither an attempt to analyze the meaning of natural-language truth predicates nor an attempt to use the notion of truth for broad philosophical purposes. Rather, Tarski's goal was to replace natural-language truth predicates with certain restricted, but formally defined substitutes. He thought such replacements were needed both to remove the doubts of certain scientifically minded truth skeptics and to eliminate what he took to be the incoherence in our ordinary notion brought out by the liar paradox.

For Tarski, these two motivations were connected, since the par-

[5] There are, I presume, many versions of realism, of which (6) and (7) represent only one. A different version might hold that some sentence of English is such that it is metaphysically possible for it to be true (keeping the semantics fixed) in cases in which the proposition it expresses cannot (ever) be known or rationally believed. This thesis is no more linguistic than (6) and (7) are.

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adoxes constituted one source of skepticism about truth.[6] However, the truth skeptics of his day also had other, more broadly philosophical grounds for their doubts. These included the frequent use of truth in metaphysical discussions, the tendency to confuse truth with epistemological notions like certainty and confirmation, and the inability to see how acceptance of a truth predicate could be squared with the doctrine of physicalism and the unity of science.[7] Although Tarski's work was historically effective in alleviating each of these worries, the only one discussed by Tarski was the final one, involving physicalism.[8]

Tarski's version of physicalism was a moderate one, allowing both physical and mathematical elements, without requiring the latter to be reduced to the former. Roughly, this "moderate physicalism" asserts that

(i) all facts are physical or mathematical facts;
(ii) all scientific (or descriptive) claims are reducible to claims about the physical and mathematical characteristics of things; and
(iii) all scientific (or descriptive) concepts are definable in terms of physical and mathematical concepts.[9]

Tarski took this doctrine to require that truth be eliminable via an explicit, physicalistic definition. Anything else—for example, taking truth to be a primitive whose extension is fixed by a set of axioms—was deemed to be undesirable.

It is worth pointing out that this emphasis on definition is primarily philosophical rather than technical. What is at issue is

[6] And also of skepticism about related notions like definability. Tarski cites the paradoxes as a source of skepticism in "The Establishment of Scientific Semantics" and, as John Burgess has pointed out to me, in "On Definable Sets of Real Numbers," reprinted in *Logic, Semantics, Metamathematics* (New York: Oxford, 1956).

[7] See C. G. Hempel, "On the Logical Positivists' Theory of Truth," *Analysis*, II, 4 (January 1935): 49–59; Rudolf Carnap, "Intellectual Autobiography," in P.A. Schilpp, ed., *The Philosophy of Rudolf Carnap* (LaSalle Ill.: Open Court, 1963), p. 63; Carnap, "Truth and Confirmation," in H. Feigl and W. Sellars, eds., *Readings in Philosophical Analysis* (New York: Appleton-Century-Crofts, 1949); Hans Reichenbach, *Experience and Prediction* (Chicago: University Press, 1938), sec. 22; Otto Neurath, "Sociology and Physicalism" and "Protocol Sentences", both reprinted in A. J. Ayer, *Logical Positivism* (Glencoe Ill.: Free Press, 1959); Karl Popper, *The Logic of Scientific Discovery* (New York: Harper Torchbook, 1965), p. 274; and Hartry Field, "Tarski's Theory of Truth," this JOURNAL, LXIX, 13 (July 13, 1972): 347–375; page references to Field will be to this article.

[8] "The Establishment of Scientific Semantics," *op. cit.*, p. 406.

[9] Tarski's physicalism countenanced both physical science and "logic," where the latter was construed as including set theory and everything obtainable from it. In what follows, I will use the term 'physicalism' in the moderate sense of (i–iii) above. In particular, physicalism, in my sense, does not require the reduction of set-theoretical facts, mathematical facts, or syntactic facts about expression types.

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not the technical results achievable, but the philosophical significance of those results.[10] It is possible to view a Tarski truth characterization for a language L as simply specifying the extension of 'true' for L, explaining how the truth value of a sentence depends on the semantic properties of its parts, and providing the basis for accounts of logical truth and logical consequence. Even if the truth characterization is put in the form of what is technically an explicit definition, it doesn't have to be viewed as an explication of truth in any interesting philosophical sense. If one's philosophical views differ from Tarski's, one can accept his formal results while taking truth to be primitive.

I will not comment directly on this way of viewing Tarski, but will instead concentrate on his own view of his work. I do this not out of any commitment to physicalism, but rather out of a sense that his deflationist attitude toward truth is interesting and worth defending. Tarski's attitude needs defense because his definition of truth fails to satisfy certain initially plausible demands one might place on an explication of truth. His attitude is defensible because these demands turn out to be dubious or illegitimate. The importance of this defense extends beyond Tarski to the general question of what ought, and what ought not, to be expected from a theory of truth.

II

Tarski's basic idea is that for certain languages L, adequate for natural science, one can define a truth predicate using only notions already expressible in L, plus certain syntactic and set-theoretic apparatus. Thus, if L is physicalistically pure and if syntax and set theory are unproblematic, then defining a metalanguage truth predicate can't introduce any difficulties.

Following Hartry Field, we can think of such a definition as divided into two parts. The first part is concerned with what Field calls "primitive denotation"; here one defines what it is for a name to refer to an object and for a predicate to apply to one or more objects. The second part of the definition defines truth in terms of primitive denotation. The end result is a metalanguage sentence:

(8) For all sentences s of L, s is true iff $T(s)$.

[10] There are, of course, technical issues as well. When the metalanguage contains quantifiers ranging over arbitrary subsets of the domain of the object language, an explicit definition of object-language truth is possible in the metalanguage. On the other hand, if a classical object language containing set theory has quantifiers ranging over all sets, then an explicit metalanguage definition of truth is impossible. Tarski's emphasis on explicit definition is philosophical in the sense that he saw significant philosophical advantages in explicit definitions of truth, *where they are possible.*

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in which $T(s)$ is a formula with only 's' free, made up entirely of logical, set-theoretic, and syntactic apparatus, plus translations of the primitives of L. If these translations are (extensionally) correct, then $T(s)$ will be coextensive with 'true' over L.

Tarski's technique can be illustrated using a particularly simple example. Let L be a language whose only logical constants are '$\vee$' and '$-$', and whose nonlogical constants consist of finitely many names and one-place predicates. (R) and (A) define reference and application for L; (T) uses these notions to define truth:

(R) For all names n of L and objects o, n refers in L to o iff $n =$ 'a' and $o =$ Arizona, or $n =$ 'b' and $o =$ Boston, . . . (and so on for each of the names of L).

(A) For all one-place predicates P of L and objects o, P applies in L to o iff $P =$ 'C' and o is a city, or $P =$ 'S' and o is a state, . . . (and so on for each one-place predicate of L).

(T) For all sentences S of L, S is true in L iff $S \in$ the set K such that for all x, $x \in K$ iff

(i) $x = \ulcorner Pn \urcorner$ for some predicate P and name n of L, and there is an object o such that *n refers in L to o* and *P applies in L to o*; or

(ii) $x = \ulcorner (A \vee B) \urcorner$ for some formulas A and B of L, and $A \in K$ or $B \in K$; or

(iii) $x = \ulcorner -A \urcorner$ for some formula A of L and $A \notin K$.

Let (T′) be just like (T) except for containing the right-hand sides of (R) and (A) where (T) contains *n refers in L to o* and *P applies in L to o*, respectively. (T′) is then an explicit Tarskian truth definition for L, with '$T(s)$' in (8) representing the right-hand side of (T′).[11]

Although truth definitions for richer languages are technically more interesting, their philosophical status as putative physicalistic reductions of truth is essentially the same as that of the simple definition just given. On the basis of such definitions, Tarski concluded that he had shown truth, reference, and application to be physicalistically acceptable terms.

In a well-known critique of Tarski, Hartry Field (*op. cit.*) argues that this conclusion is unjustified. The problem, according to Field, is that the proposed replacements for the notions of primitive denotation are not physicalistically acceptable reductions of

[11] Note, since there are only finitely many atomic formulas in L, that we could have got an equivalent result by substituting (i′) for (i) in (T).

(i′) $x =$ 'Ca' and Arizona is a city, or $x =$ 'Cb' and Boston is a city, or $x =$ 'Sa' and Arizona is a state, or $x =$ 'Sb' and Boston is a state, . . . (and so on for each atomic formula).

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our pretheoretic notions of reference and application. Because Field takes Tarski to have reduced truth to primitive denotation (350), he concludes that Tarski has not legitimated the notion of truth for physicalists.

Field does not, of course, dispute the fact that Tarski's definitions are extensionally correct. He maintains, however, that extensional correctness is not enough. In addition, any genuine reduction must show semantic facts about expressions to be supervenient on physical facts about their users and the environments in which they are used. Tarski's definitions don't do this.

This can be seen by considering a simple example. Suppose that '*Cb*' is a sentence of *L* and that the relevant semantic facts about it are given in (9):

(9) a. '*b*' refers (in *L*) to Boston.
b. '*C*' applies (in *L*) to cities (and only cities).
c. '*Cb*' is true (in *L*) iff Boston is a city.

If Tarski's definitions really specify the physicalistic content of semantic notions, then, in each case, we ought to be able to substitute the physicalistic definiens for the semantic definiendum without changing the physical fact thereby specified. Performing this substitution and simplifying results, we obtain

(10) a. '*b*' = '*b*' and Boston = Boston.
b. For all objects *o*, '*C*' = '*C*' and *o* is a city, iff *o* is a city.
c. '*Cb*' = '*Cb*' and there is an object *o* such that *o* = Boston and *o* is a city, iff Boston is a city.

But there is a problem in identifying these facts with those in (9). As Field points out, it is natural to suppose that the expressions of a language have semantic properties only in virtue of the ways they are used by speakers. Thus, he holds that the facts given in (9) wouldn't have obtained if speakers' linguistic behavior had been different.[12] Since the facts in (10) are not speaker-dependent in this way, Field concludes that they are not semantic facts and that Tarski's attempted reduction fails. Tarski's truth predicate is both physicalistic and coextensive with 'true in *L*'; but it is not, according to Field, a physicalistic conception of truth.

On Field's view, Tarski's truth characterization inherits its inadequacy as a reduction from the pseudo-reductions that constitute its base clauses. Thus, Field's strategy for solving the problem is to

[12] I use the phrase 'linguistic behavior' in a broad sense to include all facts about speakers relating to their use of language.

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provide genuine reductions for the notions of primitive denotation, on something like the model of the causal theory of reference. The picture that emerges from his discussion is one in which an adequate definition of truth is a two-stage affair. Stage 1 is Tarski's reduction of truth to primitive denotation. Stage 2 is the imagined causal theory-like reduction of the notions of a name referring to, and a predicate applying to, an object in a language.[13] If the physical facts that determine denotation in one language do so in all, then these relations will hold between expressions and objects, for variable '*L*'. When logical vocabulary and syntax is kept fixed, the result is a notion of truth that is not language-specific, but is itself defined for variable '*L*'.

Although the resulting picture appears rosy, there are several problems with it. One concerns reference to abstract objects, for which a causal account seems problematic. Another involves Quinean worries about ontological relativity and referential indeterminacy. These, of course, are obstacles to a physicalistic reduction of primitive denotation. However, there are other difficulties which become clear when one notices that Field has understated his objection to Tarski. If the alleged dependence of semantic facts on facts about speakers shows that Tarski has not reduced primitive denotation to physical facts, then the very same point shows that he has not reduced truth to primitive denotation.

This can be seen by considering a pair of elementary examples. Imagine two languages, L_1 and L_2, which are identical except that in L_1 the predicate '*R*' applies to round things, whereas in L_2 it applies to red things. Owing to this difference, certain sentences will have different truth conditions in the two languages.

(11) a. '*Re*' is true in L_1 iff the earth is round.
b. '*Re*' is true in L_2 iff the earth is red.

Under Tarski's original definition, this difference will be traceable to the base clauses of the respective truth definitions, where the applications of predicates are simply listed.

Field's objection to this is that although Tarski's definitions correctly *report* that '*R*' applies to different things in the two languages, they don't *explain* how this difference arises from the way in which speakers of the two languages use the predicate. What Field fails to point out is that exactly the same objection can be

[13] Field also includes the notion of a function sign being fulfilled by a pair of objects. In the interest of simplicity, I am ignoring this.

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brought against Tarski's treatment of logical vocabulary and syntax in the recursive part of his definition.

This time let L_1 and L_2 be identical except for their treatment '$\vee$'.

(12) a. A formula $\ulcorner(A \vee B)\urcorner$ is true in L_1 (with respect to a sequence s) iff A is true in L_1 (with respect to s) or B is true in L_1 (with respect to s).

b. A formula $\ulcorner(A \vee B)\urcorner$ is true in L_2 (with respect to a sequence s) iff A is true in L_2 (with respect to s) and B is true in L_2 (with respect to s).

Owing to this difference, sentences containing '$\vee$' will have different truth conditions in the two languages. In order to satisfy Field's requirements on reduction, it is not enough for a truth characterization to report such differences. Rather, such differences must be explained in terms of the manner in which speakers of the two languages treat '$\vee$'.[14] Since Tarski's truth definitions don't say anything about this, their recursive clauses should be just as objectionable to the physicalist as the base clauses.

This means that Field's strategy of achieving a genuine reduction of truth by supplementing Tarski with nontrivial definitions of primitive denotation cannot succeed. The reason it can't is that, given Field's strictures on reduction, Tarski has not reduced truth (for standard first-order languages) to primitive denotation. At best he has reduced it to the class of semantic primitives listed in (13):[15]

(13) the notion of a name referring to an object
the notion of a predicate applying to objects
the notion of a formula being the application of an n-place predicate P to an n-tuple of terms $t_1 \ldots t_n$
the notion of a formula A being a negation of a formula B
the notion of a formula A being a disjunction of formulas B and C
the notion of a formula A being an existential generalization of a formula B with respect to a variable u and a domain D of objects

[14] Presumably, speakers of L_1 differ in some way from speakers of L_2 regarding their beliefs, intentions, attitudes, brain states, or conditioned responses involving '$\vee$'.

[15] Field partially anticipates this point in footnotes 5 and 10 of his paper. In fn 5 he notes that in model theory quantifiers are given an "unusual" semantics in which they range over the members of some specified set, rather than over all (actually existing) things. In such a case, Field claims, Tarski has reduced truth to primitive denotation, plus the notion of the range of the quantifiers. (For Tarski this constituted the usual case, since it is only when the range of quantifiers is restricted that explicit truth definitions can be given—for languages with a certain minimal richness.)

In fn 10 Field notes, without specifying, the existence of problems that must be faced when the definition of truth is generalized so as not to contain a particular logical vocabulary.

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This way of looking at things requires a restatement of every clause in Tarski's truth definition. For example, the recursive clause for negation, which had been given by (14a), is now given by (14b).

(14) a. If $A = \ulcorner -B \urcorner$, then A is true in L (with respect to a sequence s) iff B is not true in L (with respect to s).

b. If A is a negation of a formula B, then A is true in L (with respect to a sequence s) iff B is not true in L (with respect to s).

The resulting abstraction extends the generality of the truth definition to classes of first-order languages that differ arbitrarily in syntax, plus logical and nonlogical vocabulary.

Although this generality is appealing, it has a price. Whereas the original definitions simply stipulated that $\ulcorner -A \urcorner$ is a negation, $\ulcorner A \vee B \urcorner$ is a disjunction, and $\ulcorner \exists x A x \urcorner$ is an existential generalization over a range D of objects, the revised definition doesn't provide a clue about which formulas fall into these categories. Moreover, Field's physicalist now has to provide reductions of each of these semantic notions.

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How might this be done? We are accustomed either to using truth to explain the logical notions or to taking them as primitive, while stipulating that certain symbols are to count as instances of them. Neither of these policies is open to Field. He cannot characterize negation as a symbol that attaches to a formula to form a new formula that is true (with respect to a sequence) iff the original is false (with respect to the sequence); for that would make the reduction of truth to the notions in (13) circular. Nor can he take negation to be primitive and stipulate that $\ulcorner -s \urcorner$ is to be the negation of S; for that would fail to give the facts about speakers that explain the semantic properties of $\ulcorner -s \urcorner$. Although there are alternative approaches, none that I know of is clearly successful.[16] For example, in *The Roots of Reference*[17] Quine attempts to characterize truth-functional operators in terms of community-wide dispositions to assent and dissent. He ends up concluding that indeterminacy between classical and intuitionist construals of the connectives is inevitable. Although I do not accept Quine's argument for this,[18] I do think that the task confronting Field's physicalist is nontrivial. The problems involved in reducing primitive denotation to

[16] The most interesting, in my opinion, is briefly sketched in Gilbert Harman, "Beliefs and Concepts," *Proceedings of the Philosophy of Science Association*, II (1982).

[17] LaSalle, Ill.: Open Court, 1974.

[18] For an excellent critique of Quine, see Alan Berger, "Quine on 'Alternative Logics' and Verdict Tables," this JOURNAL, LXXVII, 5 (May 1980): 259–277.

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physical facts are hard enough; adding the logical notions makes the job that much harder.

As I have stressed, the source of this difficulty is the demand that semantic facts be supervenient on physical facts about speakers. In effect, this demand limits adequate definitions to those which legitimate substitution for semantic notions in contexts like (15) and (16).

(15) If *L*-speakers had behaved differently (or been differently constituted), then '*b*' wouldn't have referred (in *L*) to Boston, and '*C*' wouldn't have applied (in *L*) to cities, and '*Cb* ∨ *Ca*' wouldn't have been true (in *L*) iff Boston was a city or Arizona was a city.

(16) The fact that *L*-speakers behave as they do (and are constituted as they are) explains why '*b*' refers (in *L*) to Boston, etc.

Field's critique of Tarski is based on the conviction that there ought to be a way of spelling out (15) and (16) so that they come out true when physicalistic substitutes replace semantic terms and their initial clauses are construed as expressing contingent physical possibilities.[19] As we have seen, Tarski's definition doesn't have this character.

III

It is helpful in understanding the issues at stake to compare this criticism of Tarski to a parallel objection. Whereas Field's critique is based upon a view about the relationship between speakers and semantic properties like truth, the parallel objection is based on a view about the relationship between meaning and truth. It is widely held that the meaning of a sentence is closely related to its truth conditions and that knowledge of the one constrains knowledge of the other. Thus, many philosophers would accept arbitrary instances of (17) and (18):

(17) If '*S*' had meant in *L* that *p*, then '*S*' would have been true in *L* iff *p*.

(18) If *x* knows that it is not the case that '*S*' is true in *L* iff *p*, then *x* knows (or has sufficient grounds for concluding) that '*S*' does not mean in *L* that *p*.[20]

[19] In stating this requirement in terms of the replacement of a semantic term by its physicalistic definiens, I have tacitly relied on Tarski's emphasis on explicit definition. However, I don't think the philosophical point of the requirement depends on this. In cases in which only an axiomatic treatment is possible, Field could require that the axioms governing 'true', together with empirical facts about speakers and their environments, have statements of type (15) and (16) as consequences.

[20] (18) is considerably weaker than the claim that knowledge of truth conditions is sufficient for knowledge of meaning. (18) says only that knowledge of truth condi-

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A natural demand growing out of this view is that substituting an adequate explication for 'true in L' in (17) and (18) should result in true sentences with contingent antecedents.[21]

As before, it is obvious that Tarski's definition does not satisfy this demand. For example, let '*Ws*' be a sentence of L meaning that snow is white. Using Tarski's definition of truth, we can produce the following counterparts of (17) and (18):[22]

(17_T) If '*Ws*' had meant in L that snow is black, then it would have been the case that snow was white iff snow was black.

(18_T) If x knows that it is not the case that snow is white iff snow is black, then x knows (or has sufficient grounds for concluding) that '*Ws*' does not mean in L that snow is black.

These are clearly not what the defender of (17) and (18) has in mind. The reason they aren't is that Tarski's set-theoretic truth predicate doesn't impose any conditions on the meanings of the sentences to which it applies. To be sure, Tarski wouldn't count any predicate T as a truth predicate unless $\ulcorner \alpha$ is $T \urcorner$ were materially

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tions is capable of providing some information about meaning. In effect, it says that even if knowledge that

(i) 'S' is true in L iff q.

is not sufficient for knowing that

(ii) 'S' means in L that q.

it should be sufficient for knowing that

(iii) 'S' does not mean in L that p.

(where the sentences replacing 'p' and 'q' are obviously incompatible).

[21] Although the contexts in question are intensional, this demand does not require that an adequate explicatum for the pretheoretic notion of truth be intensionally equivalent to an ordinary, pretheoretic truth predicate. Rather, it requires that all legitimate theoretical purposes served by the explicandum (truth), be equally well served by the explicatum. For example, if knowledge of that expressed by

(i) . . . is true . . .

is used to help explain the nature of some capacity (say, the capacity to understand sentences), then knowledge of that expressed by

(ii) . . . T . . .

(where the explicatum T is substituted for 'is true') should be sufficient for the same purpose. An explication that meets this requirement of theoretical productivity will allow the explicandum to be eliminated from one's total scientific and philosophical theory without loss of explanatory power. Thus, substitution of explicatum for explicandum in intensional contexts contained in one's total explanatory theory must be countenanced, even if such substitution is not always countenanced in ordinary discourse.

The qualification in fn 19 above regarding substitution and explicit definition also applies here.

[22] (17_T) and (18_T) are simplifications of the sentences that would result from substituting Tarski's explicatum [the right-hand side of (T') in section II] for 'true in L' in (17) and (18). The simplifications are based on the fact that, where T is Tarski's explicatum, $\ulcorner$'Snow is white' is T$\urcorner$ and 'Snow is white' are necessarily equivalent (in the presence of elementary set theory). In light of this equivalence, replacing one with the other should not affect the philosophical issues at stake in (17) and (18).

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equivalent to any metalanguage paraphrase of the object-language sentence named by α. On the basis of this, one might interpret Tarski as implicitly supposing that instances of (19) are necessary or apriori.

(19) If 'T' is a truth predicate for L, and 'S' means in L that p, then 'S' is T iff p.

However, this is quite different from maintaining that if 'T' in (20) is replaced with a truth predicate for L, then the resulting instances of the schema will be necessary or apriori:

(20) If 'S' means in L that p, then 'S' is T iff p.

It is this that the advocate of (17) and (18) demands and that Tarski appears not to provide.[23]

[23] Hilary Putnam has used a version of the argument involving (17)/(17$_T$) against Tarski (in a lecture at Princeton, fall 1982). Michael Dummett has used a version of the argument involving (18)/(18$_T$) against Tarski [in the preface to *Truth and Other Enigmas* (Cambridge, Mass.: Harvard, 1978), and in "Truth," reprinted there].

The arguments given above are intended as stand-ins for a variety of related arguments, all designed to show that Tarski's notion of truth has nothing to do with semantic interpretation or understanding. For example, it is probably best to understand Davidson not as attempting to analyze meaning in terms of truth, but rather as eliminating the notion of meaning in favor of the notion of truth. Since (18) utilizes the notion of meaning, a defender of the Davidson of "Truth and Meaning" might want to trade it for something like (i):

(i) If x knows that which is expressed by the relevant instance of
'S' is true in L iff p
for each sentence of L, then x is a competent speaker of L.

If 'true in L' is understood as short for the definiens provided by Tarski, (i) is as absurd as (18$_T$).

Just this sort of absurdity is present in familiar and often repeated remarks like the following (which would allow Tarski's definiens to be the central notion in a theory of meaning):

(T) s is T if and only if p

What we require of a theory of meaning for a language L is that without appeal to any (further) semantical notions it place enough restrictions on the predicate 'is T' to entail all sentences got from schema T when 's' is replaced by a structural description of a sentence of L and 'p' by that sentence. . . .

It is worth emphasizing that the concept of truth played no ostensible role in stating our original problem. That problem, upon refinement, led to the view that an adequate theory of meaning must characterize a predicate meeting certain conditions. It was in the nature of a discovery that such a predicate would apply exactly to the true sentences. I hope that what I am doing may be described in part as defending the philosophical importance of Tarski's semantical concept of truth [Donald Davidson, "Truth and Meaning," in J. F. Rosenberg and C. Travis, eds., *Readings in the Philosophy of Language* (Englewood Cliffs, N.J.: Prentice-Hall, 1971), pp. 455/56].

Earlier statements of essentially the same absurdity can be found in Rudolf Carnap's *Meaning and Necessity* (Chicago: University Press, 1947), pp. 5/6; and in section 7 of his *Introduction to Semantics* (Cambridge, Mass.: Harvard, 1943).

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IV

We have, then, two major objections to Tarski. Field demands that semantic properties be dependent on speakers in a way in which Tarski's substitutes are not. A familiar sort of semantic theorist demands that meaning and truth conditions be contingent, but analytically connected, properties of a sentence in a manner incompatible with Tarski. The only way to defend Tarski's philosophical interpretation of his work is to reject these demands.

Although this might initially seem to be a desperate strategy, it is not. Think of a standard first-order language L as a triple $\langle S_L, D_L, F_L \rangle$, where S_L is a family of sets representing the various categories of well-formed expressions of L; D_L is a domain of objects; and F_L is a function that assigns objects in D_L to the names of L, subsets of the domain to one-place predicates of L, and so on.[24] Let J be a class of such languages. Truth can now be defined in nonsemantic terms for variable 'L' in J in a straightforward Tarskian fashion. The only significant change from before is that the notions of primitive denotation are no longer given language-specific list definitions, but rather are defined for variable 'L' using the "interpretation" functions built into the languages. In particular, a name n refers to an object o in a language L iff $F_L(n) = o$.[25] The resulting truth predicate is just what is needed for metatheoretical studies of the nature, structure, and scope of a wide variety of theories.

What the truth definition does not do is tell us anything about the speakers of the languages to which it applies. On this conception, languages are abstract objects, which can be thought of as bearing their semantic properties essentially. There is no possibility that expressions of a language might have denoted something other than what they do denote; or that the sentences of a language might have had different truth conditions. Any variation in semantic properties (across worlds) is a variation in languages. Thus, semantic properties aren't contingent on anything, let alone speaker behavior.

[24] This sort of construction is familiar from model theory. However, its use here is different from model-theoretic treatments. Here we are not defining *truth in L relative to a model*, but rather *truth in L* (simpliciter) for an enriched conception of a language. This way of looking at things was suggested to me from two sources: David Lewis's "Languages and Language," in K. Gunderson, ed., *Minnesota Studies in the Philosophy of Science*, VII (Minneapolis: U of Minnesota Press, 1975), pp. 3–35; and one of Saul Kripke's seminars on truth, Princeton, 1982.

[25] Note, F_L is a purely mathematical object—a set of pairs, if you like. Thus, it does not incorporate any undefined semantic notions. This was one of the points noted by Kripke in the seminar mentioned in fn 24.

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What is contingent on speaker behavior is which language a person or population speaks and which expression a given utterance is an utterance of. Let L_1 and L_2 be two languages in J which are identical except for the interpretations of certain nonlogical vocabulary—perhaps the color words in L_1 are shape words in L_2. We can easily imagine a situation in which it is correct to characterize L_1, rather than L_2, as the language of a given population. To ask what such a characterization amounts to, and what would justify it, is to ask not a semantic question about the languages, but a pragmatic question about their relation to speakers.

Although Tarski had nothing to say about this relation, other philosophers have. David Lewis, using a different, but equally abstract, conception of language has proposed (*op. cit.*) an analysis in terms of a convention of truthfulness and trust. Discussions of what Donald Davidson calls "radical interpretation" can also be reconstructed as dealing with this issue. For physicalists, the interesting question is whether any purely physical explication can be given. If so, then the physicalist can accept both semantic notions that apply to sentences and those which apply to utterances. If not, then either the latter or physicalism itself must be abandoned.

It is interesting to note that much of Hartry Field's concern is with the semantic properties of utterances rather than sentences. In describing the physicalist's position he says:

> People utter the sounds 'Electrons have rest mass but photons don't' . . ., and we apply the word 'true' to their utterances. We don't want to say that it is a primitive and inexplicable fact about those utterances that they are true, a fact that cannot be explicated in non-semantic terms; this is as unattractive to a physicalist as supposing that it is a primitive and inexplicable fact about an organism at a certain time that it is in pain (*op. cit.*, 359).

In effect, Field criticizes Tarski for not providing a physicalistically acceptable truth predicate of utterances. But Tarski wasn't concerned with utterances. Thus, confronted with the question

(i) In virtue of what are certain sounds utterances which are true in L?

Tarski's response ought to be to break it up into two subsidiary questions:

(ii) In virtue of what are certain sounds utterances in L of its sentences?
(iii) In virtue of what are sentences of L true (in L)?

Whereas Tarski answered the second question, the first was no part of his task.

It is hard to see how Field himself could avoid this division of

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labor. At one point he suggests that in order to handle ambiguous and indexical expressions, truth definitions should be formulated in terms of tokens rather than types (351–353). The idea is that utterances are contextually disambiguated and that semantic notions should apply to unambiguous entities. This means that all clauses in a truth definition must be formulated as applying to tokens. To this end, Field reformulates the clause for negation as (21):

(21) A token of $\ulcorner -e \urcorner$ is true (with respect to a sequence) iff the token of e that it contains is not true (with respect to the sequence) (352).

However, this won't do. As I indicated earlier, Field can't accept any truth definition in which a certain syntactic form is simply stipulated to be a negation; for to do this would be to fail to explicate the facts about speakers in virtue of which negative constructions have the semantic properties they do. Instead, (21) must be replaced with something along the lines of (22).

(22) A token of a formula A, which is a negation of a formula B, is true (with respect to a sequence) iff some designated token of B is not true (with respect to the sequence).

But now there is a problem. Even if the notion of a formula A being a negation of a formula B can be given a physicalistic definition in terms of the behavior of speakers, there is no clear way of specifying the relevant token of B needed in (22); indeed, there is no way of ensuring that it will exist.

If we could count on utterances of negative sentences always containing, as proper parts, utterances of the sentences they are negations of, then the problem would not arise. Although this is a feature of certain artificial languages, it is not a characteristic of natural languages actually spoken by people. In order to avoid arbitrarily restricting truth definitions to (utterances involving) this subset of artificial languages, we need some way of eliminating undue dependence on empirically unreliable tokens. The most straightforward way of doing this is to define truth for types, thereby acknowledging the theoretical division of labor I have attributed to Tarski.[26] Once this is done, the physicalist is free to accept Tarski-like truth definitions applying to sentences, while leaving it open

[26] Ambiguity can then be treated as a case of homonomy. For example, instead of thinking that English contains a single (ambiguous) word type 'bank', one can take English to contain two different words, 'bank_1' and 'bank_2', whose tokens are phonologically identical. The contextual factors that Field relies on to disambiguate tokens can then be thought of as determining whether particular utterances are tokens of the type 'bank_1' or the type 'bank_2'.

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whether the pragmatic relations between languages, expressions, speakers, and utterances are purely physicalistic.[27]

It should be emphasized that although the linguistic threat to physicalism has been moved from semantics to pragmatics, it is still a serious one. It is by no means evident that physicalistic reductions of the crucial relations can be given. One physicalist who seems to think they cannot be given is Quine. Although he doesn't conceptualize matters in just the way that I have, it is illuminating to interpret him as accepting Tarski's semantic definitions while rejecting any physicalistic reduction of the pragmatic notions. On this interpretation, there is no indeterminacy about the claim that 'rabbit' refers to rabbits in a certain specified language, call it "English", or about the claim that 'gavagai' refers to rabbits in another language, call it "Junglese". What is indeterminate is whether I speak English, as opposed to some related rabbit-stage language, and whether the native speaks Junglese, as opposed to some similar counterpart.

The upshot of this is that it is all right for a Quinean physicalist to *use* a Tarskian language to describe the world, and even to attribute Tarskian semantic properties to expressions in that language. What he cannot do is identify the language he is using. When it comes to describing linguistic behavior—even one's own—identifiable Tarskian languages are excluded in favor of dispositions to verbal behavior. The strain in this position is a measure of the challenge that language use presents to physicalism. What is not problematic is the physicalist's acceptance of Tarski.

V

This discussion illustrates a general strategy for answering Tarski's critics. Field's objection was that Tarski's semantic properties are not dependent on facts about speakers. The Tarskian reply is that nothing is lost by thinking of semantics abstractly and relegating the interpretation of speakers' behavior to pragmatics. In so doing, one gains the advantages of a truth predicate for metatheoretical discussions, while retaining the ability to raise deep philosophical problems in other areas.

As I pointed out earlier, Field's is not the only objection to Tarski. Any theory of semantic competence that makes knowledge of truth conditions the central notion implicitly rejects Tarski's claim to have provided a notion of truth adequate for all theoreti-

[27] Acknowledging the need to formulate truth definitions in terms of types does not force one to think of the semantic properties of sentences as invariant from world to world and not dependent on the properties of tokens. However, it does make this a natural position.

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cal purposes. The defense against this objection is that such theories are flawed in any case.

The problem with these theories lies in specifying what truth conditions are in such a way that knowledge of them is necessary and sufficient for understanding. If we assume that truth conditions involve the notion of truth, then it is natural to suppose that they are given by T-sentences of the form (23):

(23) '*S*' is true in $L \equiv P$.

(Instances are formed by replacing '*P*' with a sentence that means the same as the sentence replacing '*S*'.) However, it is easy to show that knowing the propositions expressed by T-sentences is neither necessary nor sufficient for understanding meaning (where 'true' is taken to be a non-Tarskian primitive and '≡' represents either material or necessary equivalence). Thus it is not obvious that what one knows when one understands a language involves the notion of truth at all. If it doesn't, it may be that nothing is lost by adopting a Tarski-like explication of truth together with an independent account of semantic competence.

Although I won't try to show it here, I think that this is the right approach for both truth and semantic competence. This does not mean that Tarski's semantic predicates really are adequate for all theoretical purposes. Saul Kripke's theory of truth is a genuine advance on Tarski's treatment of the liar.[28] In addition, semantic predicates for richer languages, as well as for propositions, are needed. What does seem right about Tarski's approach is its deflationist character. Theories of truth for sentence types need not specify the facts about speakers in virtue of which their utterances have content; nor should such theories be seen as issuing in theorems knowledge of which is necessary and sufficient for semantic competence. Instead, theories, or definitions, of truth should provide accounts of the content of familiar truth predications, while resolving the semantic paradoxes (and their propositional variants). Beyond this, and the attendant dissolution of confusions, it is best not to expect too much. Truth is a useful notion, but it is not the key to what there is, or to how we represent the world to ourselves through language.

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[28] "Outline of a Theory of Truth," this JOURNAL, LXXII, 19 (Nov. 6, 1975): 690–716.

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TO BE IS TO BE A VALUE OF A VARIABLE (OR TO BE SOME VALUES OF SOME VARIABLES)*

ARE quantification and cross reference in English well represented by the devices of standard logic, i.e., variables $x,y,z,\ldots$, the quantifiers $\forall$ and $\exists$, the usual propositional connectives, and the equals sign? It's my impression that many philosophers and logicians think that—on the whole—they are. In fact, I suspect that the following view of the relation between logic and quantificational and referential features of natural language is fairly widely held:

No one (the view begins) can think that the propositional calculus contains all there is to logic. Because of the presence in natural language of quantificational words like 'all' and 'some' and words used extensively in cross reference, like 'it', 'that', and 'who', there is a vast variety of forms of inference whose validity cannot be adequately treated without the introduction of variables and quantifiers, or other devices to do the same work. Thus everyone will concede that the predicate calculus is at least a part of logic.

Indispensable to cross reference, lacking distinctive content, and pervading thought and discourse, *identity* is without question a logical concept. Adding it to the predicate calculus significantly increases the number and variety of inferences susceptible of adequate logical treatment.

And now (the view continues), once identity is added to the predicate calculus, there would not appear to be all that many valid inferences whose validity has to do with cross reference, quantification, and generalization which cannot be treated in a satisfactory way by means of the resulting system. It may be granted that there are certain valid inferences, involving so-called "analytic" connections, which cannot be handled in the predicate calculus with identity. But the validity of these inferences has nothing to do with quantification in natural language, and it may thus be doubted whether a logic that does nothing to explain their validity is thereby deficient.

In any event (the view concludes), the variety of inferences that

* I am grateful to Richard Cartwright, Helen Cartwright, James Higginbotham, Judith Thomson, and the editors of the *Journal of Philosophy* for helpful comments, criticism, and discussion. Helen Cartwright's valuable unpublished Ph.D. dissertation, "Classes, Quantities, and Non-singular Reference" (University of Michigan, 1963) deals at length with many of the issues with which the present paper is concerned.

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cannot be dealt with by first-order logic (with identity) is by no means as great or as interesting as the variety that can be handled by the predicate calculus, even without identity, but not by the propositional calculus.

It is the conclusion of this view that I want to take exception to. (At one time I thought the whole view was probably true.) It seems to me that we really do not know whether there is much or little in the province of logic that the first-order predicate calculus with identity cannot treat. In the first part of this paper I shall present and discuss some data which suggest that there may be rather more than might be supposed, that there may be an interesting variety both of quantificational and referential constructions in natural language that cannot be represented in standard logical notation and of valid inferences for whose validity these constructions are responsible. Whether quantification and cross reference in English are well represented by standard logic seems to me to be an open question, at present.

Several kinds of constructions, sentences, and inferences that cannot be symbolized in first-order logic are known. Perhaps the best-known of these involve numerical quantifiers such as 'more', 'most', and 'as many', e.g., the inference

> Most Democrats are left-of-center.
> Most Democrats dislike Reagan.
> Therefore, some who are left-of-center dislike Reagan.

Another is the construction "For every *A* there is a *B*," which, although it might appear to be symbolizable in first-order notation, cannot be so represented, for it is synonymous with "There are at least as many *B*s as *A*s."[1] The construction is not of recent date; it is exemplified in a couplet from 1583 by one T. Watson:[2]

> For every pleasure that in love is found,
> A thousand woes and more therein abound.

Jaakko Hintikka has offered a number of examples of sentences that cannot, he claims, be represented in first-order logic.[3] One of these is:

> Some relative of each villager and some relative of each townsman hate each other.

[1] Cf. my "For Every *A* There Is a *B*," *Linguistic Inquiry*, XII (1981): 465–467.
[2] See the entry for 'for' in the Oxford English Dictionary.
[3] "Quantifiers vs. Quantification Theory," *Linguistic Inquiry*, V (1974): 153–177.

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There appears to be a consensus regarding this sentence, viz., that if it is O.K., then it can be symbolized in standard first-order logic as follows:

$$\forall x \forall y \exists z \exists w (Vx \,\&\, Ty \rightarrow Rzx \,\&\, Rwy \,\&\, Hzw \,\&\, Hwz \,\&\, z \neq w)$$

I find this sentence marginally acceptable at best and not acceptable if not symbolizable as above.

Jon Barwise has offered "The richer the country, the more powerful is one of its officials" as another example of a sentence that cannot be symbolized in first-order logic.[4] However, since the sentence seems to me, at any rate, to mean "Whenever x is a richer country than y, then x has (at least) one official who is more powerful than *any* official of y," it also seems to me to have a first-order symbolization:

$$\forall x \forall y ([Cx \,\&\, Cy \,\&\, xRy] \rightarrow \exists w [wOx \,\&\, \forall z (zOy \rightarrow wPz)])$$

Are there better examples?

Perhaps the best-known example of a sentence whose quantificational structure cannot be captured by means of first-order logic is the Geach-Kaplan sentence, cited by W. V. Quine in *Methods of Logic*[5] and *The Roots of Reference*[6]:

(A) Some critics admire only one another.

(A) is supposed to mean that there is a collection of critics, each of whose members admires no one not in the collection, and none of whose members admires himself. If the domain of discourse is taken to consist of the critics and Axy to mean "x admires y," then (A) can be symbolized by means of the *second*-order sentence:

(B) $$\exists X (\exists x\, Xx \,\&\, \forall x \forall y [Xx \,\&\, Axy \quad x \neq y \,\&\, Xy])$$

And since (B) is not equivalent to any first-order sentence, (A) cannot be correctly symbolized in first-order logic.

The proof, due to David Kaplan, that (B) has no first-order equivalent is simple and exhibits an important technique in showing nonfirstorderizability: Substitute the formula $(x = 0 \vee x = y + 1)$ for Axy in (B), and observe that the result:

(C) $$\exists X (\exists x\, Xx \,\&\, \forall x \forall y [Xx \,\&\, (x = 0 \vee x = y + 1) \rightarrow x \neq y \,\&\, Xy])$$

[4] "On Branching Quantifiers in English," *Journal of Philosophical Logic*, VIII, 1 (February 1979): 47–80.

[5] 4th ed. (Cambridge, Mass.: Harvard, 1982), p. 293, where "people" is substituted for "critics" in the example.

[6] LaSalle, Ill.: Open Court, 1973; p. 111.

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is a sentence that is true in all nonstandard models of arithmetic but false in the standard model.[7]

I must confess to a certain ambivalence regarding the Geach-Kaplan sentence. Although it usually strikes me as a quite acceptable sentence of English, it doesn't invariably do so. (The "only" seems to want to precede the "admires" but the intended meaning of the sentence forces it to stay put.) I find that if the predicates in the example are changed in what one might have supposed to be an inessential way matters are improved slightly:

Some computers communicate only with one another.
Some Bostonians speak only to one another.
Some critics are admired only by one another.

I don't have any idea why replacing the transitive verb 'admires' by a verb or verb phrase taking an accompanying prepositional phrase helps matters, but it does seem to me to do so.

I turn now from this brief survey of known examples of sentences not representable in first-order logic to examination of some other nonfirstorderizable sentences. Like the Geach-Kaplan sentence but unlike the sentences involving 'most', these sentences *look* as if they "ought to be" symbolizable in first-order logic. They contain plural forms such as 'are' and 'them', and it is in large measure because they contain these forms that they cannot be represented in first-order logic.

Consider first the following sentence, which, however, contains no plurals and which can be symbolized in first-order logic:

(D) There is a horse that is faster than Zev and also faster than the sire of any horse that is slower than it.

Quantifying over horses, and using 0, s, $>$, and $<$ for 'Zev', 'the sire of', 'is faster than', and 'is slower than', respectively, we may symbolize (D) in first-order logic:

(E) $$\exists x(x > 0 \,\&\, \forall y[y < x \rightarrow x > s(y)])$$

[7] To see that (C) is true in any nonstandard model, take as X the set of all nonstandard elements of the model. X is nonempty, does not contain 0, hence contains only successors, and contains the immediate predecessor of any of its members. To see that it is false in the standard model, suppose that there is some suitable set X of natural numbers. X must be nonempty: if its least member x is 0, let $y = 0$; otherwise $x = y + 1$ for some y. Since x is least, y is not in X, and 'Xy' is false. The nonfirstorderizability of "For every A there is a B" can be established in a similar way: Select variables x and y not found in any presumed first-order equivalent, substitute $[(1) < x + 5 \,\&\, \sim\exists y 3 \cdot y = (1)]$ for $A(1)$, substitute $[(1) < x + 5 \,\&\, \exists y 3 \cdot y = (1)]$ for $B(1)$, and existentially quantify the result with respect to x; the result would be true in all nonstandard models but false in the standard model.

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Sentence (F), however, cannot be symbolized in first-order logic:

(F) There are some horses that are faster than Zev and also faster than the sire of any horse that is slower than them.

(F) differs from (D) only in that some occurrences in (D) of the words 'is', 'a', 'horse', and 'it' have been replaced by occurrences of their plural forms 'are', 'some', 'horses', and 'them'. The content of (F) is given slightly more explicitly in:

(G) There are some horses that are all faster than Zev and also faster than the sire of any horse that is slower than all of them.

I take it that (F) and its variant (G) can be paraphrased: there is a nonempty collection (class, totality) X of horses, such that all members of X are faster than Zev and such that, whenever any horse is slower than all members of X, then all members of X are faster than the sire of that horse.[8] (F) and (G) can be symbolized by means of the second-order sentence (domain and denotations as above):

$$(H)\ \exists X(\exists x\, Xx \,\&\, \forall x(Xx \rightarrow x > 0) \,\&\, \forall y[\forall x(Xx \rightarrow y < x) \rightarrow \forall x(Xx \rightarrow x > s(y))])$$

(H) is equivalent to no first-order sentence; for it is false in the standard model of arithmetic (under the obvious reinterpretation) but true in any nonstandard model, since the set of nonstandard elements of the model will always be a suitable value for X. Thus (F) cannot be symbolized in first-order logic.[9]

(F) is not an especially pretty sentence. It is hard to understand, awkward, and contrived. But ugly or not, it is a perfectly grammatical sentence of English, which has, as far as I can see, the meaning given above and no other. Moreover, such faults as it has appear to be fully shared by (D).

Another example, shorter and perhaps more intelligible:

(I) There are some gunslingers each of whom has shot the right foot of at least one of the others.

[8] Zev won the Kentucky Derby in 1923.

[9] Cf. my "Nonfirstorderizability Again," to appear in *Linguistic Inquiry*, XV, 2 (1984). In an important unpublished manuscript entitled "Plural Quantification," Lauri Carlson has given "If some numbers all are natural numbers, one of them is the smallest of them," as an example of a sentence that cannot be symbolized in the first-order predicate calculus. I have heard it claimed that this is not a proper sentence of English. Perhaps it is not, but "If there are some numbers all of which are natural numbers, then there is one of them that is smaller than all the others," surely is. I am grateful to Irene Heim for calling this reference to my attention.

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(I) may be rendered in second-order logic:

(J) $$\exists X(\exists x\, Xx \,\&\, \forall x[Xx \rightarrow \exists y(Xy \,\&\, y \neq x \,\&\, Bxy)])$$

(Here we quantify over gunslingers and use B for "has shot the right foot of.") By substituting $x = y + 1$ for Bxy, we may easily see that (J) is equivalent to no first-order sentence. (Alternatively, we may note that if we negate (J), substitute $y \leq x$ for Bxy, and make some elementary transformations, we obtain:

$$\forall X(\exists x\, Xx \rightarrow \exists x[Xx \,\&\, \forall y(Xy \,\&\, y \leq x \rightarrow y = x)])$$

a formula that expresses the least-number principle, which is one version of the principle of mathematical induction.)

When used as a demonstrative pronoun, 'that' is marked for number, as singular, but when used as a relative pronoun, as in (F), it is unmarked for number, i.e., can be used in either the singular or plural. 'Who', 'whom', and 'whose', however, are unmarked for number when used either as relative or as interrogative pronouns. 'Which' is also unmarked for number as a relative pronoun, but 'which ones', when it can be used, is strongly preferred to 'which' as an interrogative plural form; it may well be that interrogative 'which', like demonstrative 'that', is marked as singular.

It is the plural forms in (F) and (I), as well as the unmarkedness of 'that' and 'whom', that are responsible for the nonfirstorderizability of these sentences. And by taking a cue from the well-known second-order definitions of "x is a standard natural number" and "x is an ancestor of y," we can use plurals to define these notions in English (in terms of "zero" and "successor of" and in terms of "parent of," respectively):

(K) If there are some numbers of which the successor of any one of them is also one, then if zero is one of them, x is one of them.

(L) If there are some persons of whom each parent of any one of them is also one, then if each parent of y is one of them, x is one of them; and someone is a parent of y.

There are some comments on (K) and (L) to be made: (a) 'which' and 'whom' are used in these sentences as we have noticed they can be used, in the plural. (b) Instead of saying "of which the successor of any one of them is also one," one could as well say "of which the successor of any one is also one of them": at least one "them" is needed to cross-refer to the "witnessing" values of 'which': this 'them' is sometimes called a *resumptive* pronoun, and appears to be needed to capture the force of $\forall y(Xy \rightarrow Xs(y))$, with its two occurrences of X. (c) Like (F) and (I), (K) and (L) cannot be given cor-

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rect first-order symbolizations, and thus the following (valid) inference cannot be represented in first-order logic:

> If there are some persons of whom each parent of any one of them is also one, then if each parent of Yolanda is one of them, Xavier is one of them; and someone is a parent of Yolanda.
> Every parent of someone red is blue.
> Every parent of someone blue is red.
> Yolanda is blue.
> Therefore, Xavier is either red or blue.

(To see that this is a valid inference, consider the persons who are either red or blue. By the second and third premises, every parent of any one of these persons is also one of them; and since Yolanda is blue, each of her parents is red, hence red or blue, and hence one of these persons. Thus Xavier is also one of them and thus either red or blue.) (d) The 'there are's in the antecedents of course express universal quantification, as does the 'there is' in "If there is a logician present, he should leave." (e) Like (F), (K) and (L) are somewhat ungainly, in part because of the resumptive 'them' they contain, but principally because of the complexity of the thoughts they express. However, they seem to be perfectly acceptable vehicles for the expression of those very thoughts. And although they are indeed contrived—they have been contrived *to take advantage of referential devices that are available in English*—the fact that they are so hardly begins to bear on the question whether they are ungrammatical, unintelligible, or in some other way unacceptable.

The suggestion that it is the complexity of the thoughts expressed in (K) and (L) that is responsible for their ungainliness rather than the presence of any construction not properly a part of English draws support from the ease and naturalness with which "x is identical to y" may be defined in the same style: if there are some things of which x is one, then y is one of them too. (Or: it is not the case that there are some things of which x is one, but of which y is not one.)

Another example, of a different sort, is:

> (M) Each of the numbers in the sequence 1,2,4,8, . . . is greater than the sum of all the numbers in the sequence that precede it.

(M) states something true, which, using a mixture of logical and arithmetical notation, we can express as follows:

$$(N)\quad \forall x \forall y (Px \,\&\, y = \Sigma\{z : Pz \,\&\, x > z\} \rightarrow x > y)$$

In (N), Σ is a sign for a function from sets of objects in a domain

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to objects in that domain and attaches to a variable and a formula to form a term in which that variable is bound. Signs for such functions are simply not part of the primitive vocabulary of first-order logic, although on occasion mention of functions of this type can be paraphrased away (e.g. "the least of the numbers z such that . . . z . . ."). No one function sign of the ordinary sort can do full justice to "the sum of the numbers z such that . . . z . . . ," as can be seen by considering:

(O) Although every power of 2 is 1 greater than the sum of all the powers of 2 that are smaller than it, not every power of 3 is 1 greater than the sum of all the powers of 3 that are smaller than it.

We certainly cannot symbolize (O) as:

$$\forall x \forall y (Px \,\&\, y = f(x) \rightarrow x = y + 1) \,\&\, \sim \forall x \forall y (Qx \,\&\, y = f(x) \rightarrow x = y + 1)$$

and were we to try to improve matters by changing the second occurrence of f to an occurrence of (say) g, we should fail to depict the recurrence of the semantic primitive 'the sum of . . .' in the second conjunct of (O). Nor could any ordinary function sign express the dependencies that may obtain between predicates contained in '. . . z . . .' and those found in the surrounding context.

A short and sweet example of the same type is:

No number is the sum of all numbers.

The last example for the moment of a sentence whose meanings cannot all be captured in first-order logic is one that is again found in Quine's *Methods of Logic*—but not, this time, in the final part of the book, "Glimpses Beyond." It is the sentence (P):

(P) Some of Fiorecchio's men entered the building unaccompanied by anyone else.

On Quine's analysis of this sentence, it can be represented as $\exists x(Fx \,\&\, Ex \,\&\, \forall y[Axy \rightarrow Fy])$, where Fx, Ex, and Axy mean "x was one of Fiorecchio's men," "x entered the bulding," and "x was accompanied by y."[10] Quine states that "x was unaccompanied by anyone else" clearly has the intended meaning "Anyone accompanying x was one of Fiorecchio's men."

Quine's is certainly one reading this sentence bears: there are some Fiorecchians each of whom entered the building unaccom-

[10] Page, 197. Quine uses K, F, and H instead of F, E, and A, respectively.

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panied by anyone who wasn't a Fiorecchian. But since (P) appears, at times, to mean something like:

There were some men, see.
They were all Fiorecchio's men.
They entered the building.
And they weren't accompanied by anyone else.

it can also be understood to mean: there are some Fiorecchians each of whom entered the building unaccompanied by anyone who wasn't one of *them*. On this stronger reading, there is no asymmetry between the predicates "x was one of Fiorecchio's men" and "x entered the building," 'else' means "not one of them," and the whole can be symbolized by:

$$\exists X(\exists x\, Xx \,\&\, \forall x(Xx \rightarrow Fx) \,\&\, \forall x(Xx \rightarrow Ex) \,\&\, \forall x \forall y(Xx \,\&\, Axy \rightarrow Xy))$$

whose nonfirstorderizability can be seen in the usual way, by substituting $x > 0$ for both Fx and Ex and $x = y + 1$ for Axy.

It is because of these examples that I think that the question whether the first-order predicate calculus with identity adequately represents quantification, generalization, and cross reference in natural language ought to be regarded as a question that hasn't yet been settled.

Changing the subject somewhat, I now want to look at a number of sentences whose most *natural* representations are given by second-order formulas, but second-order formulas that turn out to be equivalent to first-order formulas.

The sentence:

(Q) There are some monuments in Italy of which no one tourist has seen all.

might appear to require a second-order formula for its correct symbolization, e.g.,

(R) $$\exists X(\exists x\, Xx \,\&\, \forall x[Xx \rightarrow Mx] \,\&\, \sim\exists y[Ty \,\&\, \forall x(Xx \rightarrow Syx)])$$

Of course, (Q) can be paraphrased:

(S) No tourist has seen all the monuments in Italy.

and this can be symbolized in first-order logic as:

(T) $$\exists x Mx \,\&\, \sim\exists y[Ty \,\&\, \forall x(Mx \rightarrow Syx)]$$

which is equivalent to (R).[11] But just as $\sim\sim p$ can sometimes be a

[11] It take it that since (S) implies that there are some monuments in Italy, but does not imply that there are tourists, the conjunct $\exists x Mx$ is indispensable.

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better symbolization than p of "It's not the case that John didn't go," e.g., if p were used to symbolize "John went," so (R) captures more of the quantificational structure of (Q) than does the equivalent (T). (Q) might appear to say that there is a (nonempty) collection of monuments in Italy and no tourist has seen every member of this collection; (S) doesn't begin to hint at collections of monuments. Nevertheless, (Q) and (S) say the same thing, if any two sentences do, and (R) and (T) are, predictably enough, equivalent.

Another example of the same "collapsing" phenomenon:

(U) Mozart composed a number of works, and every tolerable opera with an Italian libretto is one of them.

has the second-order symbolization:

(V) $$\exists X(\exists x Xx \,\&\, \forall x(Xx \rightarrow Mx) \,\&\, \forall x(Tx \rightarrow Xx))$$

But as (U) says what (W) says:

(W) Mozart composed a number of works, and every tolerable opera with an Italian libretto is a work that Mozart composed.

so (V) is equivalent to the first order

(X) $$\exists x Mx \,\&\, \forall x(Tx \rightarrow Mx)$$

The construction 'Every . . . is one of them' bears watching; suffice it for now to observe that it is a perfectly ordinary English phrase.

Collapses can also occur unexpectedly. (Through a publisher's error) the sentence:

(Y) Some critics admire one another and no one else.

meaning (approximately), "There is a collection of critics, each of whom admires all and only the *other* members of the collection," and possessing the second-order symbolization:

(Z) $$\exists X(\exists x \exists y[Xx \,\&\, Xy \,\&\, x \neq y] \,\&\, \forall x[Xx \rightarrow \forall y(Axy \leftrightarrow \{Xy \,\&\, y \neq x\})])$$

was claimed in the first American printing of the third edition of *Methods of Logic* to be a sentence incapable of first-order representation.[12] But although (Z) might appear to be susceptible to the same kind of treatment given out above, it was in fact observed by Kaplan to be equivalent to the first-order formula:

(a) $$\exists z(\exists y Azy \,\&\, \forall x[(z = x \vee Azx) \rightarrow \forall y(Axy \leftrightarrow \{(z = y \vee Azy) \,\&\, y \neq x\})])$$

[12] *Methods of Logic*, 3d ed. (New York: Holt, Rinehart & Winston, 1972), p. 238/9.

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Consider now sentence (b):

(b) There are some sets that are such that no one of them is a member of itself and also such that every set that is not a member of itself is one of them. (Alternatively: There are some sets, no one of which is a member of itself, and of which every set that is not a member of itself is one.)

By quantifying over sets and abbreviating "is a member of" by ϵ, we may use a second-order formula to symbolize (b):

(c) $$\exists X(\exists x\, Xx \,\&\, \forall x[Xx \rightarrow \sim x \epsilon x] \,\&\, \forall x[\sim x \epsilon x \rightarrow Xx])$$

(c) is obviously equivalent to (d):

(d) $$\exists X(\exists x\, Xx \,\&\, \forall x[Xx \leftrightarrow \sim x \epsilon x])$$

Let us notice that (d) immediately implies $\exists x \sim x \epsilon x$. Conversely, if $\exists x \sim x \epsilon x$ holds, then there is at least one set in the totality X of sets that are not members of themselves, and X witnesses the truth of (d). Thus (d) turns out to be equivalent to $\exists x \sim x \epsilon x$, the symbolization of an obvious truth concerning sets.

(The worry over Russell's paradox which the reader may be experiencing at this point may be dispelled by the observation that logical equivalence is a model-theoretic notion, the "sets" just referred to may be taken to be elements of the domain of an arbitrary model, and the "totalities," subsets of the domain of the model.)

In view of the near-vacuity of (b) and the fact that instances of the second-order comprehension schema $\exists X \forall x[Xx \leftrightarrow A(x)]$, including (e):

(e) $$\exists X \forall x[Xx \leftrightarrow \sim x \epsilon x]$$

are logically valid under the standard semantics for second-order logic, the collapse of (d) is not at all surprising. The rendering (d) of (b) is considerably more faithful to the semantic structure of (b) than is $\exists x \sim x \epsilon x$, however, and (b) is more nearly synonymous with (d) than with $\exists x \sim x \epsilon x$.

But can we use (c) or (d) to represent (b) at all? May we use second-order formulas like (c), (d), or (e) to make assertions about *all sets*?

Let's consider (e), which is slightly simpler than (c) or (d). (e) would appear to say that there is a totality or collection X containing all and only those sets x which are not members of themselves. Are we not here on the brink of a well-known abyss? Does not acceptance of the valid (e), understood as quantifying over all sets (with ϵ taken to have its usual meaning), commit us to the exis-

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tence of a set whose members are all and only those sets which are not members of themselves?

There are a number of ways out of this difficulty. One way, which I no longer favor, is to regard it as illegitimate to use a second-order formula when the objects over which the individual variables in the formula range do not form a set (just as it is illegitimate to use a first-order formula when there are *no* objects over which they range).[13] This stipulation keeps all instances of the comprehension principle as logical truths; it also enables one always to read the formula Xx as meaning that x is a *member* of the *set* X.

The principal drawback of this way out is that there are certain assertions about sets that we wish to make, which certainly cannot be made by means of a first-order formula—perhaps to claim that there is a "totality" or "collection" containing all and only the sets that do not contain themselves is to attempt to make one of these assertions—but which, it appears, could be expressed by means of a second-order formula if only it were permissible so to express them. To declare it illegitimate to use second-order formulas in discourse about all sets deprives second-order logic of its utility in an area in which it might have been expected to be of considerable value.

For example, the principle of set-theoretic induction and the separation (Aussonderung) principle virtually cry out for second-order formulation, as:

(f) $$\forall X(\exists x\, Xx \rightarrow \exists x[Xx \,\&\, \forall y(y \epsilon x \rightarrow \sim Xy)])$$

and

(g) $$\forall X \forall z \exists y \forall x(x \epsilon y \leftrightarrow [x \epsilon z \,\&\, Xx])$$

respectively. It is, I think, clear that our decision to rest content with a set theory formulated in the first-order predicate calculus with identity, in which (f) and (g) are not even well-formed, must be regarded as a compromise, as falling short of saying all that we might hope to say. Whatever our reasons for adopting Zermelo-Fraenkel set theory in its usual formulation may be, we accept this theory because we accept a stronger theory consisting of a *finite* number of principles, among them some for whose complete expression second-order formulas are required.[14] We ought to be able to formulate a theory that reflects our beliefs.

[13] I took this view in "On Second-order Logic," this JOURNAL, LXXII, 16 (Sept. 18, 1975): 509–527.

[14] Cf. the remarks about "full expression" and "part of the content" of various notions in my "The Iterative Conception of Set," this JOURNAL, LXVIII, 8 (April 22, 1971): 215–231.

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We of course also wish to *maintain* such second-claims as are made by e.g., $\exists X \forall x[Xx \leftrightarrow \sim x \epsilon x]$; if we are to utilize second-order logic in discourse about all sets, these comprehension principles must remain among the asserted statements. Nor do we want to take the second-order variables as ranging over some set-like objects, sometimes called "classes," which have members, but are not themselves members of other sets, supposedly because they are "too big" to be sets. Set theory is supposed to be a theory about *all* set-like objects.

How then can we legitimately maintain that such (closed) formulas as $\exists X \forall x[Xx \leftrightarrow \sim x \epsilon x]$, (f), and (g) express truths, without introducing classes (set-like non-sets) into set theory and without assuming that the individual variables do not in fact range over all the sets there really are?

There is a simple answer. Abandon, if one ever had it, the idea that use of plural forms must always be understood to commit one to the existence of sets (or "classes," "collections," or "totalities") of those things to which the corresponding singular forms apply. The idea is untenable in general in any event: There are some sets of which every set that is not a member of itself is one, but there is no set of which every set that is not a member of itself is a member, as the reader, understanding English and knowing some set theory, is doubtless prepared to agree. Then, using the plural forms that are available in one's mother tongue, translate the formulas into that tongue and see that the resulting English (or whatever) sentences express true statements. The sentences that arise in this way will lack the trenchancy of memorable aphorisms, but they will be proper sentences of English which, with a modicum of difficulty, can be understood and seen to say something true.

Applying this suggestion to:

(h) $\sim\exists X(\exists x\, Xx \,\&\, \forall x[Xx \rightarrow (x \epsilon x \vee \exists y[y \epsilon x \,\&\, Xy \,\&\, y \neq x])])$

which is equivalent to (f), we might obtain:

(i) It is not the case that there are some sets each of which either contains itself or contains at least one of the others.

From Aussonderung we might perhaps get:

(j) It is not the case that there are some sets that are such that it is not the case that for any set z there is a set y such that for any set x, x is a member of y if and only if x is a member of z and also one of them.

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or, far more perspicuously,

(k) ~ there are some sets such that
$$\sim \forall z \exists y \forall x [x \epsilon y \leftrightarrow (x \epsilon z \,\&\, x \text{ is one of them})]$$

(k) is of course neither an English sentence nor a wff of any reputable formalism—for that matter neither is (j), which contains the (non-English) variables x, y, and z—but is readily understood by anyone who understands both English and the first-order language of set theory. It would be somewhat laborious to produce a fully Englished version of (g), but the labor involved would be mainly due to the sequence $\forall x \exists y \forall x$ of *first-order* quantifiers that (g) contains. (j) and (k) are actually not quite right; properly they have the meaning:

(l) $$\sim \exists X(\exists x\, Xx \,\&\, \sim \forall z \exists y \forall x [x \epsilon y \leftrightarrow (x \epsilon z \,\&\, Xx)])$$

whereas the full Aussonderung principle omits the nonemptiness condition $\exists x\, Xx$; to get the full content in English of Aussonderung, however, we need only conjoin "and there is a set with no members" to (j) and $\exists y \forall x \sim x \epsilon y$ to (k). This observation calls to our attention two small matters connected with plurals which must be taken up sooner or later.

Suppose that there is exactly one Cheerio in the bowl before me. Is it true to say that there are some Cheerios in the bowl? My view is no, not really, I guess not, but say what you like, it doesn't matter very much. Throughout this paper I have made the customary logician's assumption, which eliminates needless verbiage, that the use of plural forms does not commit one to the existence of two or more things of the kind in question.

On the side of literalness, however, I have assumed that use of such phrases as "some gunslingers" in "There are some gunslingers each of whom has either shot his own right foot or shot the right foot of at least one of the others" does commit one to—as one might say—a *nonempty* class of gunslingers, but not to a class containing two or more of them. Thus I suppose the sentence to be true in case there is exactly one gunslinger, who has shot his own right foot, but to be false if there are aren't any gunslingers. It is this second assumption that is responsible for the ubiquitous $\exists x\, Xx$ in the formulas above.

Translation will be difficult from any logical formalism into a language such as English, which lacks a large set of devices for expressing cross reference. And since plural pronouns like 'them', although sometimes used as English analogues of second-order vari-

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ables, much more frequently do the work of individual variables, translation from a second-order formalism containing infinitely many variables of both sorts into idiomatic, flowing, and easily understood English will be impossible nearly all of the time. My present point is that, in the cases of interest to us, the things we would like to say can be said, if not with Austinian or Austenian grace.

It is, moreover, clear that if English were augmented with various subscripted pronouns, such as 'it_x', 'that_x', 'it_y', . . . , 'them_X', 'that_X', 'them_Y', . . . , then any second-order formula[15] whose individual variables are understood to range over all sets could be translated into the augmented language, as follows: Translate Vv as 'it_v is one of them_V', $v \epsilon v'$ as 'it_v is a member of $\text{it}_{v'}$', $v = v'$ as 'it_v is identical with $\text{it}_{v'}$', & as 'and', $\sim$ as 'not', and, where F^* is the translation of F, translate $\exists vF$ as 'there is a set that_v is such that F^*'.

The clause for formulas $\exists VF$ is not quite so straightforward, because of the difficulty about nonemptiness mentioned above. It runs as follows: Let F^* be the translation of F, and let F^{**} be the translation of the result of substituting an occurrence of $\sim v = v$ for each occurrence of Vv in F. Then translate $\exists VF$ as 'either there are some sets that_V are such that F^*, or F^{**}'.

For example, $(Xx \leftrightarrow \sim x \epsilon x)$ comes out as "It_x is one of them_X iff it_x is not a member of itself"; $\forall x(Xx \leftrightarrow \sim x \epsilon x)$, as "Every set is such that it is one of them_X iff it is not a member of itself"; and $\exists X \forall x(Xx \leftrightarrow \sim x \epsilon x)$, as "Either there are some sets that are such that every set is one of them iff it is not a member of itself or every set is a member of itself." (We have, of course, improved the translations as we went along.)

I want to emphasize that the addition to English of operators '$\text{it}_{()}$', '$\text{that}_{()}$', '$\text{them}_{()}$', etc. or variables 'x', 'X', 'y', etc. is not contemplated here. The 'x' of 'it_x' is not a variable but an index, analogous to 'latter' in 'the latter', or 'seventeen' in 'party of the seventeenth part'; 'X' and 'x' in 'them_X' and 'it_x' no more have *ranges* or *domains* that does '17' in 'x_{17}'. We could just as well have translated the language of second-order set theory into an English augmented with pronouns such as 'it_{17}', 'them_{1879}', etc. or an elaboration of the "former"/"latter" usage. Note also that such augmentation will be needed for the translation into English of the language of *first*-order set theory as well.

[15] We assume that no quantifier in any formula occurs vacuously or in the scope of another quantifier with the same variable; every formula is equivalent to some formula satisfying this condition.

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Charles Parsons has pointed out to me that although second-order existential quantifiers can be rendered in the manner we have described, it is curious that there appears to be no nonartificial way to translate second-order *universal* quantifiers, that the translation of $\forall X$ must be given indirectly, via its equivalence with $\sim\exists X\sim$. Because our translation "manual" relies so heavily on the phrases 'there is a [singular count noun] that is such that . . . it . . .' and 'there are some [plural count noun] that are such that . . . they . . .', the logical grammar of the construction these phrases exemplify is worth looking at.

Of course, in ordinary speech, the construction 'that is/are such that . . . it/they . . .' is almost certain to be eliminable: the content of a sentence containing it can nearly always be conveyed in a much shorter sentence. But the difference between the two 'that's bears notice. The second one, following 'such', is a 'that' like the one found in oblique contexts and may be—as Donald Davidson has suggested that the 'that' of indirect discourse is—a kind of demonstrative, used on an occasion to point to a subsequent utterance of an (open) sentence; the first 'that', following the count noun and more frequently elided than the second, is no demonstrative, but a relative pronoun used to bind the 'it' or 'they' in the open sentence after 'such that'. Thus the first but not the second 'that' works rather like the variable immediately following an $\exists$, binding occurrences of that same variable in a subsequent open formula. Whether the preceding count noun is singular or plural appears to make no difference to the quantificational role of the first 'that'; as we have observed, 'that' is not marked for number and can serve to bind either 'it' or 'they'.

Whether any such second-order formula of the sort we have been considering can be translated into intelligible *un*augmented English is not an interesting question, and I shall leave it unanswered. Since English augmented in the manner I have described is intelligible to any native speaker who understands the term of art 'party of the seventeenth part', I shall assume that devices like 'it_x' and 'them_X' are available in the language we use.

I take it, then, that there is a coherent and intelligible way of interpreting such second-order formulas as (e), (f), and (g) even when the first-order variables in these formulas are construed as ranging over all the sets or set-like objects there are. The interpretation is given by translating them into the language we speak; the translations of (e), (f), and (g) are sentences we understand; and we can see that they express statements that we regard as true: after all, we do think it false that there are some sets each of which either contains

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itself or contains one of the others, and, once we cut through the verbiage, we do find it trivial that there are some sets none of which is a member of itself and of which each set that is not a member of itself is one. It cannot seriously be maintained that we do not *understand* these statements (unless of course we really *don't* understand them, as we wouldn't if, e.g., we knew nothing at all about set theory) or that any lack of clarity that attaches to them has anything to do with the plural forms found in the sentences expressing them. The language in which we think and speak provides the constructions and turns of phrase by means of which the meanings of these formulas may be explained in a completely intelligible way.

It may be suggested that sentences like (i) are intelligible, but only because we antecedently understand statements about collections, totalities, or sets, and that these sentences are to be analyzed as claims about the existence of certain collections, etc. Thus "There are some gunslingers . . ." is to be analyzed as the claim that there is a collection of gunslingers. . . .[16] The suggestion may arise from the thought that any precise and adequate semantics for natural language must be interpretable in set theory (with individuals). How else, one may wonder, is one to give an account of the semantics of plurals?

One should not confuse the question whether certain sentences of our language containing plurals are intelligible with the question whether one can give a semantic theory for those sentences. In view of the work of Tarski, it should not automatically be expected that we can give an adequate semantics for English—whatever that might be—in English. Nothing whatever about the intelligibility of those sentences would follow from the fact that a systematic semantics for them cannot be given in set theory. After all, the semantics of the language of ZF itself cannot be given in ZF.

In any event, as we have noticed, there are certain sentences that cannot be analyzed as expressing statements about collections in the manner suggested, e.g., "There are some sets that are self-identical, and every set that is not a member of itself is one of them."

[16] In a similar vein, Lauri Carlson writes, "I take such observations as a sufficient motivation for construing *all* plural quantifier phrases as quantifiers over arbitrary *sets* [Italics Carlson's] of those objects which form the range of the corresponding singular quantifier phrases." His "Plural Quantifiers and Informational Independence," *Acta Philosophica Fennica*, xxxv (1982): 163–174, is a recent interesting article in which this claim is made once again. He is by no means the sole linguist with this belief. Carlson does not face the question of what is to be done when the corresponding singular quantifier phrase is 'some set'.

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That sentence says something trivially true; but the sentence "There is a collection of sets that are self-identical, and every set that is not a member of itself is a member of this collection," which is supposed to make its meaning explicit, says something false.

I want now to consider the claim that a sentence of English like "There are some sets of which every set that is not a member of itself is one" is actually false, on the ground that this sentence *does* entail the existence of an overly large set, one that contains all sets that are not members of themselves.

The claim that this sentence entails the existence of this large set strikes me as most implausible: there may be a set containing all trucks, but that there is certainly doesn't seem to *follow* from the truth of "There are some trucks of which every truck is one." Moreover, and more importantly, the claim conflicts with a strong intuition, which I for one am loath to abandon, about the meaning of English sentences of the form "There are some *A*s of which every *B* is one," viz. that any sentence of this form means the same thing as the corresponding sentence of the form "There are some *A*s and every *B* is an *A*." If so, the sentence of the previous paragraph is simply synonymous with the trivial truth "There are some sets and every set that is not a member of itself is a set," and therefore does not entail the existence of an overly large set.

Two worries of a different kind are that the construction "there are some [plural count noun] that are such that . . . they . . .' is unintelligible if the individuals in question do not form a "surveyable" set and that our understanding of this construction does not justify acceptance of full comprehension. I cannot deal with these worries here; I shall only remark that it seems likely that not much of ordinary, first-order, set theory would survive should either worry prove correct.

We have now arrived at the following view: Second-order formulas in which the individual variables are taken as ranging over all sets can be intelligibly interpreted by means of constructions available to us in a language we already understand; these constructions do not themselves need to be understood as quantifying over any sort of "big" objects which have members and which "would be" sets "but for" their size. There can thus be no objection on the score of unintelligibility or of the introduction of unwanted objects to our regarding ZF as more suitably formulated as a finitely axiomatized second-order theory than as an infinitely axiomatized first-order theory, whose axioms are the instances of a finite number of schemata, as is usual. (Of course, in the presence of the usual other

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first-order axioms of ZF, i.e., the axioms of extensionality, foundation, pairing, power set, union, infinity, and choice, only the one second-order axiom, Replacement:

$$\forall X(\forall x\forall y\forall z[X\langle x,y\rangle \,\&\, X\langle x,z\rangle \rightarrow y = z] \rightarrow \forall u\exists v\forall y[y\epsilon v \leftrightarrow \exists x(x\epsilon u \,\&\, X\langle x,y\rangle)])$$

would be needed.) The great virtue of such a second-order formulation of ZF is that it would permit us to express as single sentences and take as axioms of the theory certain general principles that we actually believe. The underlying logic of such a formulation would be any standard axiomatic system of second-order logic, e.g., the system indicated, if not given with perfect precision, in Frege's *Begriffsschrift*.[17] The logic would deliver the comprehension principles $\exists X\forall x[Xx \leftrightarrow A(x)]$ (which are needed for the derivation of the infinitely many axioms of the first-order version of ZF from the finitely many second-order axioms) either through explicit postulation of the comprehension schema, as in Joel Robbin's *Mathematical Logic*,[18] or via a rule of substitution, like the rule given in chapter 5 of Alonzo Church's *Introduction*[19] or the one implicit in the *Begriffsschrift*. The interpretation of this version of ZF would be given in a manner similar to that in which the interpretation of the usual formulation of ZF is given, by translation into English in the manner previously described.

Entities are not to be multiplied beyond necessity. One might doubt, for example, that there is such a thing as the set of Cheerios in the (other) bowl on the table. There are, of course, quite a lot of Cheerios in that bowl, well over two hundred of them. But is there, in addition to the Cheerios, also a set of them all? And what about the $>10^{60}$ subsets of that set? (And don't forget the sets of sets of Cheerios in the bowl.) It is haywire to think that when you have some Cheerios, you are eating a *set*—what you're doing is: eating THE CHEERIOS. Maybe there are some reasons for thinking there is such a set—there are, after all, $>10^{60}$ ways to divide the Cheerios into two portions—but it doesn't follow just from the fact that there are some Cheerios in the bowl that, as some who theorize

[17] Gottlob Frege (Hildesheim: Georg Olms Verlagsbuchhandlung, 1964).

[18] *Mathematical Logic: A First Course* (New York: W. A. Benjamin, 1969). Section 56 of Robbin's book contains a presentation of the version of set theory here advocated. It is noted there that this theory is "essentially the same as" Morse-Kelley set theory (MK), but the difficulties of interpretation faced either by MK or by a set theory in the ZF family for which the underlying logic is (axiomatic) second-order logic are not discussed.

[19] *Introduction to Mathematical Logic* (Princeton, N.J.: University Press, 1956).

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about the semantics of plurals would have it, there is also a set of them all.

The lesson to be drawn from the foregoing reflections on plurals and second-order logic is that neither the use of plurals nor the employment of second-order logic commits us to the existence of extra items beyond those to which we are already committed. We need not construe second-order quantifiers as ranging over anything other than the objects over which our first-order quantifiers range, and, in the absence of other reasons for thinking so, we need not think that there are collections of (say) Cheerios, in addition to the Cheerios. Ontological commitment is carried by our *first*-order quantifiers; a second-order quantifier needn't be taken to be a kind of first-order quantifier in disguise, having items of a special kind, collections, in its range. It is not as though there were two sorts of things in the world, individuals, and collections of them, which our first- and second-order variables, respectively, range over and which our singular and plural forms, respectively, denote. There are, rather, two (at least) different ways of referring to the same things, among which there may well be many, many collections.

Leibniz once said, "Whatever is, is one."

Russell replied, "And whatever are, are many."[20]

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BOOK REVIEWS

The Nature of Mathematical Knowledge. PHILIP KITCHER. New York: Oxford University Press, 1983. 287 p. $25.00.

Kitcher has given us two books, albeit unified by a common point of view: a historically oriented philosophy of mathematics, and a philosophically oriented history of mathematics.* The history part takes up the last four of the ten chapters of *The Nature of Ma-*

[20] Bertrand Russell, *The Principles of Mathematics*, 2d ed. (London: Allen & Unwin, 1937), p. 132.

* These reflections were greatly clarified by interchanges with Charles Parsons and the other members of his Fall, 1983, seminar at Columbia University, which I attended, and at which Kitcher's book was discussed. What I say here, however, does not necessarily reflect the others' reactions to the book. Conversations with Paul Benacerraf, Michael Resnik, and David Shatz also improved the content and the style of this review.

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thematical Knowledge. What unifies the book most of all is Kitcher's attack on a priori philosophies, a sensible refusal to cordon off mathematics from the rest of science, and a conviction that the history and the philosophy of mathematics can inform one another.

I find myself in agreement with most of what Kitcher says in the last four chapters, which is also, in my opinion, better argued than the rest. Adopting a broadly Kuhnian position, Kitcher sees the history of mathematics as the evolution of mathematical practices; and argues that mathematical knowledge develops through the "rational modification" of such practices (165). A "mathematical practice" involves such things as a language, a set of mathematical views, a set of accepted questions, a set of accepted "reasonings," and a set of accepted statements (164). Kitcher regards the a-priorist view that rational change in mathematics consists only in adding new demonstrations and finding mistakes in old ones as wrong-headed and ahistorical. In the first place, mathematical proof is not always necessary for mathematical knowledge, as Euler's work amply shows. Second, it may not always be rational (as it is not always rational in science) to attempt simply to pile up one certainty upon another. As Kitcher convincingly shows in the last chapter (which I think is the best in the book, a philosophical history of mathematical analysis from Newton to the nineteenth century), the demand for rigor may not always be rational—in fact, it may impede problem solving. As Kitcher makes clear, the philosophical objections of Bishop Berkeley to Newton's infinitesimal calculus, when taken seriously, actually impeded mathematical research; and Frege's philosophical problems with the foundations of arithmetic were rightly ignored by working mathematicians.[1] The rigorization of mathematics effected by Cauchy and Weierstrass was motivated, rightly, not by a priori epistemic concerns, but by specific research problems. Kitcher thus sees much more rationality in the actual development of mathematics than can the a-priorist, to whom the history of mathematics must look in some instances like a comedy of errors.

I certainly welcome Kitcher's philosophical treatment of the history of analysis. My only criticism is the absence of any mention in the book of nonstandard analysis—which both its inventor, Abraham Robinson, amd Imre Lakatos used as a historico-philosophi-

[1] However, Paul Benacerraf in "Frege: The Last Logicist," in *Midwest Studies in Philosophy*, vol. 6 (Minneapolis: U of Minnesota Press, 1981): 17-35, questions the common assumption that Frege's foundational work on arithmetic was motivated primarily by *philosophical* questions.

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cal tool in probing the history of mathematical analysis.[2] The rehabilitated infinitesimals of nonstandard analysis make the history of mathematics seem even more rational than Kitcher leaves it. I find no greater objection to this than to the equally anachronistic use of Fregean quantifiers which Kitcher does use in his historical analyses.

I turn now to the first six chapters of *The Nature of Mathematical Knowledge*, with whose treatment of mathematical reality and knowledge I have substantial disagreements. In these chapters, Kitcher

(a) analyzes the concept of a priori knowledge, or rather what that concept reduces to when one has already accepted a "psychologistic" or naturalistic account of knowledge and warrant;

(b) argues that none of the known philosophies of mathematics can prove the existence of a priori knowledge;

(c) suggests a neo-Millian, empiricist account of mathematical reality and mathematical knowledge, consistent with *Kitcher's own version* of the doctrine of mathematical-scientific unity;

(d) highlights the social or communal aspect of mathematical knowledge, which is just as important to Kitcher as the empirical (rudimentary mathematical knowledge in prehistory is based upon the experience of individuals; but society is intimately involved thereafter in propagating and developing such knowledge).

Kitcher makes heavy use of the social nature of mathematical knowledge in attacking what he regards as the fundamental mistake of a-priorism in the philosophy of mathematics. The view that mathematical knowledge is a priori is central to an entire array of misguided philosophies, says Kitcher. And Kitcher devotes a very large portion of the book to showing that a priori mathematical knowledge is impossible, relying repeatedly upon our willingness to consult authority in mathematics as we do everywhere else. Mathematical knowledge is vulnerable to "social challenges," for example, the scorn of our peers—and Kitcher feels that true a priori knowledge would be completely kibbitz-free. In the jargon Kitcher favors, a priori knowledge is "warranted" by some "process" which "could continue to do so, whatever experience we were to have" (63). And peer pressure, from the naturalistic or "psychologistic" stance Kitcher adopts, is just as much experience as any other.

[2] Cf. Robinson, "The Metaphysics of the Calculus," reprinted in *Selected Papers of Abraham Robinson*, vol. 2, H. J. Keisler *et al.*, eds. (New Haven, Conn.: Yale, 1979): 537–555; also Lakatos, *Mathematics, Science, and Epistemology: Philosophical Papers*, vol. 2, John Worrall and Gregory Currie, eds. (New York: Cambridge, 1978): 43–60.

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A priori knowledge, according to Kitcher, is therefore *incorrigible*—we could never be justified in giving it up once it is warranted. And perhaps it is even *unrevisable*—meaning that nothing at all could shake our conviction.[3] But this conception of a priori knowledge is to some degree a straw man, limiting the usefulness of the book. Some philosophers, indeed, have wedded their a-priorism to incorrigibility and even to unrevisability. Both Plato and Descartes, for example, thought that knowledge (Descartes calls it "certainty") of *any* type would be impervious to any kind of attack, rational or irrational. But I doubt whether either of these philosophers would have been impressed by Kitcher's appeal to "social challenges." Plato argues in the *Republic* that the truly just man (who is also the perfect knower) is "brave," by which (as he makes clear) he means "epistemically tenacious." He differs with George Orwell, who believed that "2 + 2 = 4" would be given up sincerely by anyone to whom the right kind of torture was applied. And Descartes regards certainty (whether achieved through clear and distinct ideas or by reflecting upon the Divine guarantee of nondeception) as proof even against an evil demon (or at least the thought of one, and certainly against social challenges). Both Plato and Descartes would have allowed "social challenges" to be successful against *memories* of a priori warrants, not against the warrants themselves. (Thus Descartes invokes the Deity to bolster only knowledge based upon *past* "clear and distinct ideas.")

More importantly, by concentrating upon the alleged incorrigibility of a priori knowledge, Kitcher allows himself to ignore the Kantian conception of a-priorism, according to which mathematical knowledge is simply *nonempirical* (this is, after all, what 'a priori' means according to the philosopher who defined the term). By disposing of incorrigible knowledge (at least to his own satisfaction), Kitcher believes that a priori knowledge is necessarily laid to rest—but Kant never claims that mathematical knowledge is incorrigible, only that it is necessary and nonempirical. It is not that Kitcher is unaware of such a distinction; to the contrary, he states quite openly, "One can easily go astray . . . by conflating *a priori* knowledge with knowledge obtained by following a non-empirical process" (43). But why is this going astray? We have here two notions of the a priori, of which the second is no less interesting and crucial than the first.

And it is not that Kitcher ignores Kant's philosophy of mathematics. To the contrary, he has a critical discussion of it (49ff).

[3] This particular version of the distinction I learned from Benacerraf.

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Kitcher points out that mathematical knowledge, as obtained from Kantian intuition, is open to social challenges—yet Kant (unlike Plato and Descartes) never said it wasn't. Kitcher also cites the "irrelevance problem" ("How do we distinguish between the properties of the presented entities which reflect the structure of the mind and those which are accidental?") and the "practical impossibility problem" ("How do we determine that sequences of presentations which we cannot in practice achieve are in principle possible for us?"). But these two arguments, if successful, would show not merely that Kant's philosophy of mathematics provides no basis for a priori mathematical knowledge, but that Kant's philosophy of mathematics provides no basis for mathematical knowledge of any type. In sum, Kitcher's former objection to Kant (from social challenges) is irrelevant, and the latter objections, if successful, prove "too much." Confusingly, Kitcher has no argument that, while granting Kant the possibility of mathematical knowledge *Kant's* way, attacks *merely* its a priori nature—in *Kant's* sense. More seriously, he has no general argument, not depending upon specific features of (Kitcher's interpretation of) Kant's philosophy, against the popular view that mathematics is nonempirical.

(It would only be fair to note, however, that Kitcher's conception of the a priori as entailing incorrigibility does suit the historical contexts of the end of the book. History teaches, as Kitcher shows, that the quest for incorrigible mathematical knowledge may itself be irrational! History cannot tell us, however, whether mathematics is empirical or not.)

Now what is Kitcher's own view of mathematical reality and knowledge? Mathematics begins in prehistory with elementary perceptual knowledge—perceptual knowledge, that is, of "physical operations of segregation, spatial rearrangement, and so forth" (117). This, of course, would not explain how mathematics gets to describe the infinite—our powers of perception are finite. However, Kitcher claims, an "ideal agent" can be conceived for whom the performance of infinitely many operations is child's play. Such fictions are common in science (recall the nonexistent "ideal gas" of thermodynamics), so that the objection to Kitcher's "Millian" philosophy is converted into a proof of it.

Surely Kitcher has made life too easy for himself here. As philosophers from Hilbert to Quine have made clear, one cannot invoke "ideal objects" without some kind of "bridge principles" to the real world. (Kitcher himself complains that a similar objection can be launched against the abstract entities of Platonism!) And one finds that these bridge principles typically involve a heavy use of

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mathematical entities. In Hilbert's case, the "bridge principle" needed is a consistency proof to show that the "ideal entity" may be introduced without catastrophe. (Hartry Field, in *Science without Numbers,*[4] has argued that we can have knowledge of consistency without invoking abstract entities; I am not persuaded by his arguments, but unfortunately the reasons for my recalcitrance have so far appeared only in Hebrew.[5]) In Quine's case, one must show something stronger: that the "ideal entities" may be "paraphrased away" as the limiting case of "real" entities and their properties. We can hardly satisfy either Quine or Hilbert without invoking mathematical entities.

Consider a historical use of ideal entities: Newton's use of "mass points" in his formulation of mechanics. Since there are no such things, the application of Newtonian mechanics to the observed world is problematic. Quine's way out is to say that Newton's laws assert what is more and more nearly true, the more the size of the particles of a mechanical system becomes negligible in relation to the distances between them. One could hardly paraphrase Newton's laws to reflect this interpretation without introducing mathematical entities. Furthermore, ideal entities in physical science function otherwise than as limiting cases. Newton, it is told, labored many years to calculate the gravitational force between two massive, large, neighboring spheres (the Earth and the moon, for example), until he finally proved what is today an exercise in calculus texts: that each sphere may be regarded as if all of its mass were concentrated in its center—provided that the gravitational force obeys the inverse-square law. Again, of course, the invocation of mathematical entities in stating this theorem (to say nothing of proving it) will be essential. Kitcher, however, faces a serious difficulty which can be overcome only by a careful account of the use of ideal entities in science, one which does not presuppose mathematics.

Another claim of this book which I find questionable is Kitcher's insistence that there is a problem of accounting for the usefulness of mathematics in the natural sciences which can be solved not by Platonism but by Kitcher's philosophy. In Kitcher's words, "When we draw conclusions from a mixture of scientific and mathematical premises, what accounts for our success?" (105) The answer: "The great utility of mathematics [is] explained by reference to its delineation of a structure exemplified by all physical objects" (107). What

[4] Princeton, N.J.: University Press, 1980.
[5] Review of Field, *op. cit.*, in *Iyyun,* XXXI (July 1982): 211–217.

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this means is that, as I have mentioned before, Kitcher views elementary mathematics as describing various physical operations performable by an ideal agent. Since, then, mathematics simply generalizes our everyday experience with physical bodies, there is no longer any problem about its usefulness.

But note first that there are two problems concerning the usefulness of mathematics which the philosophy of mathematics seems to face. The first is: why does the universe as a whole seem so mathematical? Why are the various theoretical and observational magnitudes of physics so often related in "mathematical" ways? (Physical systems exemplify Hilbert spaces, Lie groups, etc.) This question has been ignored by philosophers for various reasons: some deny the phenomenon to be explained; others (Kantians and evolutionists) think that the explanation lies wholly within us. I believe that there really is a mystery here; but Kitcher certainly has not solved it, even granting his empiricism. Kitcher's solution would amount to saying, like Mill, that mathematics simply generalizes our experience; yet most physical theory describes phenomena quite remote from our everyday perception of ordinary bodies. Indeed, as many philosophers have noted, the "mathematical fit" of nature to our laws becomes better and better the less observational those laws become. Kitcher and I would agree, I think, that this is something to be explained; anyhow, Kitcher has not explained it.

Kitcher's view is relevant to a second problem concerning the applicability of mathematics: how can invoking *abstract entities* aid in the explanation of events in nature? (The first problem, which I have just stated that Kitcher has *not* solved, has to do with the *concepts* of mathematics, and would survive even a total nominalization of mathematics.) Even the use of elementary arithmetic operations in explaining nature becomes an anomaly (which it was not in the first problem, since the *concept* of addition, and the applications of that concept, are suggested by nature itself)—to the extent that we presuppose, in describing these operations, to the existence of abstract entities. That Kitcher is indeed worried by this problem (and thinks that he can solve it) is shown by his words, cited above: "When we draw conclusions from a mixture of scientific and mathematical premises, what accounts for our success?"

Now Kitcher's solution, which involves a nominalist reworking of mathematics, does solve the problem. If abstract entities are not required in mathematics, we cannot worry about their usefulness in describing nature. Nevertheless, the problem can otherwise be solved—indeed, I am inclined to view it (though not the first problem) as a pseudo problem. The only "interface" between mathemat-

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ics and nature postulated in mathematical physics is that concrete objects of nature are allowed to belong to sets, which are abstract. Subsequently, "pure" mathematics is applied to the sets themselves, not to the concrete objects. Hence Kitcher's worry reduces to the following not very puzzling question: How can (abstract) sets have concrete members? The answer is: why shouldn't they? Because abstract and concrete entities do not interact causally? But who says that the set-membership relation is, or is based upon, causality (we have had the Causal Theory of Knowledge, the Causal Theory of Perception, the Causal Theory of Reference, and the Causal Theory of Memory—do we now have the Causal Theory of Relations)?[6]

At any rate, I have already expressed skepticism concerning Kitcher's appeal to "ideal agents." Ideal entities are no better than abstract entities, so long as the "bridge principles" linking the ideal entities to the real world are not specified or are specified with the help of the very abstract entities the ideal entities are supposed to replace. This is actually a lesson one learns from reading Wittgenstein: the use of "ideal calculators" is just a disguised form of Platonism which Wittgenstein attacked even more vigorously than out-and-out Platonism of the G. H. Hardy variety (with which I believe Wittgenstein sympathized, though of course he did not accept it).

Clearly, I have not been persuaded by Kitcher's arguments. Nevertheless, Kitcher's point of view cannot be ignored. And Kitcher's approach to the history of mathematics should serve notice on historians and philosophers alike that a one-sided approach to either the history or the philosophy of mathematics cannot be sustained.

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Mechanism, Mentalism, and Metamathematics: An Essay on Finitism. JUDSON C. WEBB. Boston: Reidel, 1980. xii, 277 p. $27.00.

Professor Webb's initial purpose was to refute the frequently made (and frequently refuted) claim that Gödel's incompleteness theorems refute mechanism. If you're put off by the prospect of 240 pages devoted to this well-worn subject, have no fear; most of the book is

[6] Again I am grateful to Benacerraf, for critical comments on an earlier version of this argument.

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historical: a rambling and fascinating disinterment of the mathematical and philosophical roots of mathematical logic. Rough "philosophical" beginnings are usually occluded by later "scientific" success and then forgotten; the fourteenth-century prehistory of calculus and Gallilean physics lay in obscurity for almost 600 years, bequeathing to the Johnny-come-lately scholars enormous interpretive problems. We are fortunate to have the fruits of Webb's enormous erudition available before the cobwebs thicken around the stumblings of insightful and confused pioneers as they transformed logic into the "normal science" practiced today.

Webb's concerns are remarkably broad, as a glance at his long bibliography (which itself should have significant value) shows. He has a fine eye for interesting analogies and for bad arguments. This book has its flaws, including the common sin of intellectual histories: failure to explain precisely the central "isms". The first chapter is a history of mechanism. Turing's thesis ["A procedure (function) is 'effective' just in case it can be simulated (computed) by a Turing machine"] is called "the foundation of modern mechanism" (9). Earlier (2) David Bohm's statement of "the essence of mechanism" is quoted with apparent endorsement: "the great diversity of things . . . can be reduced completely . . . to nothing more than consequences . . . of a final set of purely quantitative laws determining the behavior of a few kinds of entities"; but contemporary standards of rigor recommend further discussion of what "reducible completely" and "purely quantitative" mean here. Webb repeatedly asserts that mechanism is a form of finitism—an intriguing claim, except that 'finitism' (like 'mentalism') remains completely unexplicated. Turing's thesis is finitistic in that Turing machine-tables and convergent computations are finite objects. But the connection with Bohm's "definition" is unclear. LaPlace's world view (the universe is a system of particles governed by the deterministic laws of classical dynamics) presupposes that the fundamental physical magnitudes have the metrical structure of the continuum and that the fundamental laws are differential equations constraining continuous functions; the finitistic nature of this view is hardly obvious.

Webb points out the relevance of effectiveness to discussions of determinism and of thermodynamics. Following Bohm, he distinguishes mechanism from determinism; in fact, he says that by incorporating randomness one's world-picture becomes *more* mechanistic. This incorporation exorcizes LaPlace's demon from mechanism; since the apparent possibility of a demon who can predict all physical events weakens mechanism's plausibility, the

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mechanist's case is thereby strengthened. But if this is Webb's only argument, he's shown that incorporating randomness makes the mechanist world view more plausible, not more mechanistic!

Webb is fond of describing the dialectic of science in demonic terms. Descartes's demon (Gilbert Ryle's "ghost in the machine") is exorcized by a Hobbsean (i.e., a materialist computational) account of thinking; and of course Maxwell's demon is exorcized by Leo Szilard's well-known argument. Maxwell's demon represents the possibility that an intelligent being, within a suitable setup, could make the entropy of a system decrease; the possibility of such a violation of the second law of thermodynamics embarrasses the lawlikeness of this law. Szilard argued that this being would have to perceive, measure, and keep track of the particles making up the system, and that this physical realization of these psychological processes involves an increase of entropy, saving the second law. Webb sees here an analogue of Turing's thesis: Szilard supposes that Maxwell's demon could be replaced by a mechanism, preserving what's important in the thought experiment. He says that results of Chaitain uncover a common structure to the exorcism of Maxwell's demon and the unsolvability of the halting problem: both involve the nonexistence of an effective gambling system for answering certain questions; I'd have liked more details.

Incompleteness and unsolvability results exorcize another demon, that of Hilbert, one capable of what Webb calls "thought-free knowledge" (as Maxwell's was of "energy-free work"). Webb's description of Hilbert's demon is not completely clear; the point seems to be this: suppose *P* completely formalizes a large chunk of mathematics; given a sentence, Hilbert's demon searches for a proof in *P* of it and of its negation, and eventually decides its truth. The demon metaphor here is not as apt as elsewhere. Given seventeenth-century psychology, Cartesian demons seemed quite probable; given nineteenth-century thermodynamics, Maxwell's demons seemed quite possible; but given turn-of-the-century mathematics, how likely was a complete, or even a decidable, formalization of a large chunk of mathematics? Here Webb points out a tremendous irony: the so-called "formalists" didn't particularly expect there to be such formalizations, or for the Decision Problem to be solvable! Hilbert's thesis (S) that "Every 'definite' mathematical problem has an 'exact' solution" included the rider "which may well be negative, showing the impossibility of a positive solution"; indeed, Webb claims that the independence of the Continuum Hypothesis from ZFC was the sort of impossibility result which Hilbert would take to confirm (S)! (See pp. 111–119, 145/6.) Hilbert's demon

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saddled only the "anti-formalists" and phenomenologists (156–163): Poincaré (and Kaufmann) apparently assumed that number theory could be completely formalized; Poincaré concluded that formalization was sterile for research: if all truths are theorems, the formalization won't select the interesting theorems (hardly a compelling argument); Brouwer, we're told, thought that a constructive consistency proof for number theory would be trivial! Webb makes quite clear that the so-called "formalists" were more frequently on the right track than were their opponents. This is also apparent in their reactions to Richard's paradox, discussed in chapter three.

The second chapter runs through seventeenth-century views on our idea of infinity, early views on recursive definition, Dedekind's "proof" that there are "simply infinite systems," and reactions to it by several of his contemporaries, including C. S. Peirce (who wrote that Dedekind's book "proves no difficult theorem that I had not proved or published years before") and Josiah Royce (who saw Dedekind as vindicating Hegel on "completely self-conscious thought"). Webb points to the analogy between Dedekind's definition of infinitude (i.e., equinumerosity with a proper subset) and the geometric constructions (e.g., of a smaller regular pentagon properly contained in a given one) which led to the discovery of incommensurable ratios, thereby introducing the notion of infinity into mathematics: both involve, in Royce's words "an object that contains as a part of itself a perfect picture of itself." Later (202) he notes the analogy between Gödelian incompleteness and the incompleteness of the Greek's arithmetic of continuous magnitude: both involved the non-totality of an algorithm: that of Archimedes in one case, and of Hilbert's demon in the other case.

Dedekind's recursion theorem [if (N, O, ′) is a Peano model and (M, a, g) is any model, then there is a unique $f: \mathrm{N} \to \mathrm{M}$ such that $f(\mathrm{O}) = a$ and $f(n') = g(f(n))$] is discussed in detail. Webb claims that the obvious inductive argument ["(i) $f(\mathrm{O}) = a$, hence f is defined for O, (ii) $f(n)$ is defined $\Rightarrow f(n')$ is defined. $\therefore$ f is defined for all n."] established only the uniqueness, not the existence, of such an f. Of course Webb is right in this sense: if we view functions from N into M as objects, e.g., as sets of ordered pairs, then to establish the existence of f we'll need an axiom of infinity and some form of replacement. But he doesn't notice that if we view such functions as Fregean unsaturated entities whose existence consists only in their assigning their values to their arguments,[1] then this induction does establish, in the appropriate sense, the existence of f.

[1] See Montgomery Furth, "Two Kinds of Denotation," in Nicholas Rescher, ed., *Studies in Logical Theory*, APQ Monograph no. 2 (Oxford: Basil Blackwell, 1968).

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The long third chapter is loosely organized around Hilbert's work: his reaction to Kronecker's demand that definitions provide decision procedures, to Frege's insistence that consistency of geometric axioms doesn't guarantee their truth, what he meant by thesis (S) mentioned above, how his early work on geometry was related to his later proof-theoretic program, and the philosophic underpinnings of that program. On the Frege-Hilbert dispute, Webb seems to side with Hilbert; but he doesn't recognize the extent to which they were talking past each other: Frege was concerned with the truth (simpliciter) of interpreted (i.e., sense-expressing) sentences; Hilbert was concerned with the truth-in-a-model of disinterpreted sentences. Webb says of Hilbert's successes in basing "the algebraic treatment of elementary geometry almost entirely on (D)" (Desargues's theorem) that it "reveals a weakness in Frege's view that no meaning can be given to a geometric statement without first fixing the meanings of its constituent terms 'point' and 'line': *they tell us more of the 'meaning' of (D) than such definitions could ever hope to.*" Maybe so, construing 'meaning' as 'importance' or 'conceptual role', but not construing it as 'sense' or 'truth conditions'.

The set-theoretic paradoxes stimulated Hilbert's attempt to reconstruct Dedekind's work, replacing Dedekind's "Gedankenwelt" by a universe of syntactic objects, "individual 'thought-objects', where each such object is treated as a primitive symbol or a combination of such." Webb repeatedly describes Hilbert as wondering whether there was a finitistic version of Dedekind's isomorphism theorem (that all Peano systems are isomorphic); it's quite unclear what this means; if we read 'finitistic' as 'first order', it sounds like the question: are there categorical axioms for first-order arithmetic? But it turns out (193) that the question Webb has in mind is: "whether this (first-order) logic could logically decide every sentence of formal number theory from his (Hilbert's) axiom system." Since completeness does not imply categoricity, the initial description of Hilbert's concern is unnecessarily misleading. Within his presentation of Hilbert's foundational work, Webb discusses the reactions to Richard's paradox; this included a remarkable anticipation of Gödel's incompleteness results in a paper by Finsler (1926) which tried to use Richard's paradox (and an appeal to intensional entities) to argue that formulas could never "replace 'logical thought'." We're told that Gödel "brought Finsler's undecidable propositions down from 'rein Gedankliche' and put them back into formal systems" (151).

Webb emphasizes the Kantian spirit of Hilbert's program. Hil-

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bert saw himself as de-anthropomorphizing the transcendental aesthetic and analytic, transposing them into the following principles: (H1) "The content of intuition, which must include discrete spatial symbols as an extralogical *a priori* condition of mathematical knowledge, is always finite"; (H2) "Our mind and its content are finite; all thinking is a finite process." The claim that for Hilbert "symbols play the role of the *a priori*" is not crystal-clear; nor is the import of (H2). Webb is right in saying that "Hilbert's notion of a 'meaningless symbol' has led to considerable misunderstanding"; he takes Hilbert to be merely claiming that the connection between numerals and numbers is "arbitrary", that numerals are not "iconic" (or, using Webb's word, "mnemonic"), unlike geometric diagrams which really "picture" ideal geometric objects (142/3); but if the numerals are in unary notation, this distinction seems unmotivated. We're told that Hilbert also made a "Duhemian point about the impossibility of testing theoretical sentences in isolation from their whole theory" (142); but neither the content nor the textual basis for this claim is as clear as one would like.

The vagueness of (H2) comes home to roost when we consider the charge by Carnap and Tarski that Hilbert's introduction of the omega-rule into formal arithmentic violated (H2). Unfortunately, Webb's discussion here is very obscure. He seems to argue that Hilbert had in mind the restricted omega-rule presented by Shoenfield; by Shoenfield's theorem, he might as well have had it in mind. Shoenfield presents a "semi-formal" theory, in which a proof is still a finite object (coded by a natural number), but in which the omega-rule may be applied to infer $(x)A(x)$ provided there is a recursive function f (whose index is part of the proof-code) assigning to each premise $A(n)$ a proof (code) $f(n)$ of that premise. The set of proof (codes) is not recursive; but, we're told, "for Hilbert a 'finite' rule and one whose correct applicability is always mechanically decidable are not in general the same thing" (153). So far, so good, though the passage Webb cites as supporting this attribution does not, I think, clearly do so. Then Webb asks "does Hilbert's rule really bring anything essentially new into play beyond a machine?" I'm not sure what this means: is he trying to answer Rosenbloom, Raggio, and Freudenthal, who apparently claimed that we, unlike machines, can "grasp" proofs in such a semi-formalism? Webb defends his response by appealing to Shoenfield's result: that the restricted omega-rule proves everything the unrestricted one does, including all truths of elementary arithmetic. The relevance of this appeal is unclear. Furthermore, since proofhood is now undecidable, surely something "essentially new" *is* in play! Whatever

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Webb is up to here, he'd serve his cause best by denying that one can grasp an arbitrary proof in Shoenfield's semi-formalism.

Georg Kreisel is quoted following Brouwer in claiming that we can grasp infinite proofs; with deft irony Webb contrasts Kreisel's exhortation to "take seriously . . . what is counted as experience in traditional mental philosophy" with Brentano's view "that the very notion of the actual infinite is self-contradictory." This leads into a discussion of phenomenological treatments of infinity in the writings of Husserl and Becker (who tried to give a Husserlian basis for the work of Cantor and Gentzen). In the course of this discussion, Webb glibly asserts that "no one has seriously challenged Gentzen's claim that his proof [of the consistency of a formalized arithmetic] is finitist" (160); then in a footnote (p. 171) he acknowledges Gödel's claim in his 1958 *Dialectica* paper that ϵ_0 induction is not "unmittelbar anschaulish", "and hence that it [Gentzen's proof] was not finitistic." A similar "challenge" occurs in Bernays's article on Hilbert in *The Encyclopedia of Philosophy*. Bernays quotes Church's claim that viewing ϵ_0 induction as a boundary line for finitist reasoning is "so artificial and arbitrary as hardly to be accepted"; but one could as well say that the apparent "slippery slope" has been shown to have a natural break at ϵ_0. 'Finitistic reasoning' never had a uniform meaning, and by now is best replaced by more informative and less contentious labels. The same goes for 'finitistic function': once the relevant classes (e.g., primitive recursive, less-than-ϵ_0 recursive, general recursive) have been isolated, dispute over the honorific is idle.

The last chapter reviews the emergence of the concepts of a general recursive and a partial recursive function, and the piecemeal discoveries that various definitions of these classes coincided, in the work of Hilbert, Ackermann, Schönfinkel, Herbrand, Gödel, and Kleene. Here too Webb's discussion sometimes goes mirky. As a preface to his discussion of Church's thesis (CT) he runs through some "evident" closure properties of the "intuitively computable functions"; apparently one of these is his principle of iteration: "$f_1(x)$, $f_2(x)$, $f_3(x)$, . . . is any sequence of effective functions $\Rightarrow h(n,x) = f_n(x)$ is an effective function of two variables" (177). But this is plausible only if the sequence is itself effective; and what could that amount to but the truth of the consequent? From such closure properties Webb argues to an analogue of the recursion theorem for any "hypothetical 'system' of effective functions" of a certain form (179); this result is said to "lead to the strongest arguments known for Church's thesis" (180). Webb presents Kleene's recursion theorem for the partial recursive functions, de-

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fined by the obvious relaxation of Gödel's definition of a general recursive function. He then says this theorem (i) provides "that basic protection needed for (CT), namely the undecidability results," and (ii) figures in the proofs that the Gödelian definition of a general recursive function coincides with a multitude of other definitions. This latter coincidence is, of course, the main reason for the widespread acceptance of (CT); but how does Webb's analogue of the recursion theorem for his systems of intuitively effective functions figure here? Webb calls the incompleteness results "the guardian angels" of (CT) and, hense, of (T) and mechanism, since they block a refutation of (CT) by diagonal definition of an obviously effective but nonrecursive function; but the only point here is that a complete (even for only Π^0_1 sentences) formalization of arithmetic would permit such diagonalization; such a "block" to a refutation hardly counts as evidential support. Webb's point is in part that the unsolvability of the halting problem "underlies" the undecidability of a Gödel sentence; to rely on the recursion theorem to prove the former involves unnecessary labor. Webb points out that the usual proof of the former result "is already built into the proof" of the recursion theorem; but then it becomes unclear exactly what exalted role he's assigning to the recursion theorem. In any case, the analogue of the recursion theorem for Webb's systems of intuitively effective functions again seems to play no role. Work by Wagner and Friedman on axiomatizing informal recursion theory is mentioned, but not used; Webb suggests that these efforts "weaken the basis for the common opinion that (CT) could never be proved" (216); progress in this area (by Moschovakis and Gandy) since 1970 is not mentioned.

Webb seems to endorse the popular view that the cybernetical thesis (C) ("Every 'precisely described' human behavior can be simulated by a suitably programmed computer") "is, or boils down to" (CT). Construing 'simulate' as 'duplicate', (C) makes a strong claim about hardware: the "suitable" program would have to be one for robotic control of a humanoid (or even human) body; and, if I understand the use of 'computer' in (C), an embodied brain doesn't count as one. A weaker reading of 'simulate' seems to leave (C) vacuous or hopelessly vague. To his credit, Webb does recognize that (C) is independent of the (scarcely intelligible) thesis: "Man is a universal Turing machine." He presents Hao Wang's doubt as to whether each "humanly effective computation procedure" is "mechanistically effective", Kreisel's (and Gödel's) doubt as to whether the human computer (or mind) is capable of only finitely many internal (or mental) states, and Gödel's reported belief

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that because a mechanistic basis for biology would make life so improbable, it's unlikely to have such a basis. He answers these points well.

He also presents and replies well to the silly arguments of Lucas (230–233) and an obscure argument of Kreisel (194–196) against mechanism, always emphasizing that the Gödel sentence of a theory is mechanically computable from an effective presentation of its axioms. Some (e.g., Weyl) have said that incompleteness results show that "we cannot formalize our intuitive number concept." In response, Webb points to some complete theories of the natural numbers; he asks whether "as long as we are content to just add our numbers we can keep them in perfect axiomatic focus, but that as soon as we start to add and multiply them they somehow suddenly elude such focus" (200). The point is well taken: the universe of "the intended model" of arithmetic is not the source of incompleteness. He also says that undecidable sentences are "long and far away from ordinary mathematical experience . . . known only through a metamathematical code giving them a meaning which, although 'unnatural', is more manageable that their natural number-theoretical meaning" (202); this may hold for Gödel's original examples, but after the results of Paris and Harrington, the author should have known better.

Webb quotes enormous quantities of material, some essential to his presentation, in the original German, without translation or paraphrase; he also liberally sprinkles his own prose with German expressions; if you're not fluent in German, you'd better have reading knowledge and a German-English dictionary "zuhanden". Furthermore, he seems to presuppose a good background in mathematics, especially for his long (and quite interesting) discussion of Hilbert's work in geometry. Frequent reference is made to axioms of congruence, of continuity, of space, etc.; the axioms could easily have been formulated in footnotes. In a long discussion of Desargue's theorem, the basic concept of two triangles being in perspective is not defined until well after it is used. If you're not up on geometry, keep a contemporary geometry text nearby. Sometimes Webb goes into great detail on "technical" matters, and sometimes he skirts details so that only those who know them will follow him. He doubtless thought he had to strike a balance between writing a slim volume for a small audience and writing a fat book for a large audience; the latter would have been better, for I fear he's ended up with a middle-sized book for a small audience.

Cornell University HAROLD T. HODES

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THE JOURNAL OF PHILOSOPHY

VOLUME LXXXI, NO. 9, SEPTEMBER 1984

REFERENCE EXPLAINED AWAY

Anaphoric Reference and Indirect Description

THIS paper presents a construction by means of which I believe it is possible to account for the use of expressions containing 'refers' and its cognates in natural languages. Reference comes in two varieties: word-*world* or extralinguistic reference of the sort invoked when we say that the phrase 'the first left-handed President' refers to a certain concrete individual (namely James Garfield), and word-*word* intralinguistic or anaphoric reference instanced by pronouns such as 'she' referring to the word 'Mary' in the sentence "Mary tired of surgical training, so she became an anesthesiologist." Intralinguistic reference of this sort has not been of much interest to philosophers, for it has seemed natural to assimilate pronouns to bound variables, and so to expect to explain anaphoric reference as grammatically guaranteed *co*reference. This coreference is in turn thought of as understood in terms of the primary, word-world sense of reference to the same extralinguistic item.

I will argue that an analysis of anaphoric mechanisms provides the resources for a purely *intra*linguistic account of the use of the English sentences by means of which philosophers attempt to make assertions about *extra*linguistic referential relations. More specifically, although we can and must distinguish between our words and what those words refer to or have as their referents, the truth of claims about what we are referring to by various utterances gives us no reason to believe that there is a *relation* of reference between expressions and the objects we use them to talk about. Following the lead of Wilfrid Sellars,[1] I will argue that 'refers' should

[1] For instance "Truth and 'Correspondence'," this JOURNAL, LIX, 2 (Jan. 18, 1962): 29–56, and *Science and Metaphysics* (London: Routledge & Kegan Paul, 1963; New York: Humanities, 1968).

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not be semantically interpreted by or as a relation and, *a fortiori*, not a word-world relation. Instead, 'refers' will be explained as a complex anaphoric pronoun-forming operator, by analogy (in the category of terms) to the analysis of '. . . is true' offered by the prosentential theory of truth. To show this we first investigate the anaphoric roles that expressions can play. This leads to the specification of a new part of speech, *indirect descriptions*. Next, a formal test is offered for identifying expressions playing the anaphoric role of indirect descriptions, and 'refer' is explained as an operator that forms such descriptions. A paraphrase strategy is offered by means of which reference claims ostensibly of other forms can be wrestled into forms in which 'refer' appears only inside indirect descriptions. Finally, it is argued that doing this should be seen as explaining reference away.

I

In his seminal article "Reference and Context" Charles Chastain[2] suggests a novel approach to the understanding of singular-term reference. The basic concept he employs is that of an *anaphoric chain*, a notion best approached by example. Consider the discourse:

(1) #A man in a brown suit approached me on the street yesterday and offered to buy my briefcase. When I declined to sell it, the man doubled his offer. Since he wanted the case so badly, I sold it to him.#

Two anaphoric chains are intertwined here, one corresponding to the buyer, and one to the briefcase:

(2) A man in a brown suit . . . the man . . . he . . . him

and

(3) my briefcase . . . it . . . the case . . . it

The reference of later elements in such chains (e.g., 'it' and 'the man') is secured only by the relation these elements stand in to the singular terms that initiate the chains in which they appear. This is the word-word (in fact, token-token[3]) relation of *anaphoric reference* or *anaphoric dependence*. The presence of an anaphoric chain in a discourse signals that not all its singular terms have reference

[2] In Keith Gunderson, ed., *Minnesota Studies in the Philosophy of Science*, vol. VII: Language, Mind, and Knowledge (Minneapolis: U of Minnesota Press, 1975), pp. 194–269.

[3] To be exact, it is token*ings* which stand in anaphoric relations, although I haven't been fussy about this distinction in the text. Written expressions are particularly vulnerable to re-use of tokens, as when someone makes a sign with an arrow saying "He is a sinner," and walks around pointing it at passersby.

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independently. Rather, some elements are related to their referents only in a derivative manner, in virtue of their anaphoric links to other expressions.

Examining the kinds of expression that can initiate and continue such chains enables Chastain to make two important related observations. The first concerns the significance of indefinite descriptions. Since Russell's discussions early in the century, indefinite descriptions have been treated as if they were not singular referring expressions at all, but were rather to be understood by means of a quantificational paraphrase. The presence of an indefinite description often does signal existential quantification rather than singular reference, but Chastain points out that the role of indefinite descriptions in anaphoric chains indicates that these expressions can also have a purely referential function. As in the example above, an indefinite description can initiate an anaphoric chain which may then be continued by pronouns or definite descriptions. And it seems clear that, in the context in which it occurs above, 'a man' purports to refer to a unique individual, namely the man in the brown suit who approached me on the street yesterday and eventually purchased my briefcase.[4]

This observation leads to Chastain's second point, which is that the reason that apparently nonquantificational uses of indefinite descriptions have not been thought of as straightforwardly referential is that they do not behave enough like proper names, the paradigm of singular terms. Except under deviant circumstances, if a proper name is used somewhere in a discourse invoking a particular referent, then other tokens of that same type which appear elsewhere in the discourse will be coreferential with it, in a sense which can be explained in terms of intersubstitution. In

(4) #Leibniz has been called a pluralist, and he has been called a monist. But no one has ever thought of that philosopher as a materialist.#

the sense is not altered if we replace all the other elements of the anaphoric chain by the initiating expression to which they anaphorically refer. An inelegant redundancy is the only cost of replacing 'he' and 'that philosopher' by 'Leibniz'.

[4] Not all elements of anaphoric term chains need be understood as singular referring terms. Chastain says that quantificational, modal, and hypothetical contexts are "referentially segregating" and that the syntactically singular expressions that occur inside them should not in general be understood as singular referring terms. The claims of this essay don't require special treatment of such segregated occurrences.

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In the case of an anaphoric chain initiated by an indefinite description, on the other hand, such a substitution of terms alters the sense of the sentences in which the substituted terms appear. Consider:

(5) #A Republican senator threatened to filibuster the Wilderness bill. The senator's staff persuaded him that this action was unwise, so he left the chamber.#

The anaphoric chain here is

(6) A Republican senator . . . The senator . . . him . . . he

The sense of this discourse is completely altered if we substitute the initiating expression for each of the terms that anaphorically refers to it:

(7) #A Republican senator threatened to filibuster the Wilderness bill. A Republican senator's staff persuaded a Republican senator that this action was unwise, so a Republican senator left the chamber.#

In this passage the indefinite descriptions do not purport to corefer. Each initiates a potentially new anaphoric chain. To continue such a chain requires either the use of a pronoun, which always continues an existing chain, or the use of a definite description, which can either initiate or continue a chain. The fact that a chain beginning 'a SORTAL . . .' cannot be continued by repeating the initiating phrase as is, but can be continued with a definite description of the form '*the* SORTAL . . .' is called by linguists the requirement of a "definitization transformation." One may conclude from such special requirements either that indefinite descriptions are not referring expressions or that not all referring expressions must behave like proper names. Chastain's suggestion is that we explore the second alternative.[5]

II

Singular-term tokens can play various roles in anaphoric chains. Such a token may initiate an anaphoric chain, as 'A Republican senator' does in (5). Or it may continue an existing chain and so depend for its referent upon an anaphoric antecedent, as "the senator" does in (5). Besides distinguishing tokens as anaphoric initiators and dependents, we can sort them according to two distinctions regarding the term types they instantiate. Chastain's considerations concern-

[5] As Chastain points out, at the least this approach requires us to amend the simple intersubstitutability model of coreference, and the correlative distinction between extensional and nonextensional contexts.

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ing substitution show that in dealing with anaphoric chains one may not in general assume that cotypical term tokens are coreferential, even in the absence of overtly indexical elements. Let us call those expressions which [according to substitution tests such as that of comparing (5) and (7) above] do not vary in reference from token to token within the type, *type-substitution invariant*. An example would be proper names as conceived and idealized by the tradition. Expressions that are referentially variable from token to token within the type may be described as not cotypically intersubstitutable. Pronouns would be a paradigm.[6]

The third distinction it will be useful to make is that between lexically complex expressions and those which are lexically simple, though perhaps grammatically complex; that is, between phrases that are nouns and words that are nouns. Consider, for instance, two varieties of cotypically nonintersubstitutable anaphoric dependents: dependent definite descriptions and personal pronouns. The lexically simple pronoun 'he' is limited, in the information it can give about its anaphoric antecedent and the chain of which it is a part, to a small number of dimensions such as gender and number, specified in advance by the grammar of the language. Lexically complex anaphoric dependents, on the other hand, can use the full descriptive resources of the language to give anaphoric information. This open-endedness permits dependents such as 'the senator mentioned above, who opposed the Wilderness bill and was dissuaded by his staff from expressing his stand'. The same contrast of lexical complexity applies to anaphoric initiators; so we may compare the indefinite description 'A Republican senator' with the proper name 'Leibniz' in (5) and (4).

Deploying these three independent functional distinctions—between anaphoric initiating tokens and dependent tokens, between type-substitution-invariant types and cotypically nonintersubstitutable types, and between lexically complex types and lexically simple types—yields eight roles that tokens can be thought of as playing in anaphoric chains. Thus among anaphoric initiators that are type-substitution invariant there are those which are lexically simple, such as proper names like 'Leibniz' in (4), and

[6] It would be less clumsy to call the expressions not all cotypical tokens of which are coreferential "token reflexive." I do not, because that phrase has an established usage and is not generally thought of as applying to expressions like 'the senator', even when such expressions are used as anaphoric dependents, which are among the paradigmatic cotypically nonintersubstitutable term occurrences. It may be that the special substitution conditions applying to anaphorically dependent expressions is more explanatorily fundamental than indexicality of canonical token-reflexives.

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those which are lexically complex, such as 'the first U.S. President'. Among the cotypically nonintersubstitutable anaphoric initiators there are again the lexically simple, such as 'this', and lexically complex indefinite descriptions, such as 'A Republican senator' in (5). Among the anaphoric dependents that are not cotypically intersubstitutable one can similarly distinguish lexically simple pronouns such as 'it' from lexically complex dependent uses of definite descriptions such as 'the man' in (1). Finally, among the type-substitution-invariant anaphoric dependents one can distinguish some uses of lexically simple proper names, as in

#I met a man I'll call "Binkley." Binkley is a mechanic.#

from lexically complex dependents, which will be called *indirect definite descriptions.*

With the exception of the last category mentioned, this tripartite division just rearranges familiar facts about the linguistic behavior of standard kinds of singular terms. The categorization was presented, however, to introduce the notion of indirect definite descriptions, a kind of singular term whose existence has not generally been recognized. Indirect definite descriptions are accordingly characterized as lexically complex, type-substitution-invariant anaphoric dependents. From this specification it follows that expressions in this category are complex pronouns, as are ordinary anaphorically dependent definite descriptions. Unlike such descriptions, however, all cotypical tokens of expressions in this category are guaranteed to be coreferential with each other, since they all anaphorically depend upon and hence corefer with a single common antecedent token, and so with each other. The idea is that an indirect definite description is a pronoun which actually contains a description specifying the term occurrence that is its anaphoric antecedent. Cotypical tokens of an indirect-definite-description type will contain the same description, and so (except in special cases) specify the same antecedent. One immediate expressive advantage of a language containing locutions of this sort would be that identities employing anaphorically indirect descriptions could be used to assert that two term *tokens* were coreferential, even if the tokens were of cotypically nonintersubstitutable types (such as indefinite descriptions or pronouns) for which, as Chastain showed, standard substitutional accounts of coreference fail since they presuppose type-substitution invariance.

A useful picture of the functioning of these expressions, the picture that motivates our calling them *indirect* descriptions, is offered by the indirect addressing function offered in most basic computer architectures. Ordinarily, the central processor uses addresses to

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pick out values, just as we use descriptions to pick out objects. But, in indirect mode, the CPU when given an address as an input does not return the value stored at that address as its output. Instead it treats that value as another address and returns the value stored in that second address as its output. Indirect descriptions are to be understood by analogy to this two-step process. First, a token is specified, perhaps by being described as to type and spatiotemporal location. But the token thus picked out is not the referent of the whole indirect description. For next, an indirectness operator is applied to that token specification to produce the indirect description, which only *anaphorically* refers to the specified token and so, as a whole, refers not to that token, but rather to whatever that token, its anaphoric antecedent, refers to, just as with ordinary pronouns. The flexibility of the von Neumann computer architecture is in large part due to its capacity to treat the same expression both as datum (that is, as a value) and as instruction (the address at which the value can be found). Indirect descriptions exploit the analogous use/mention amphibiousness made possible by anaphora, and in this fact, I want to claim, consists the expressive dividend *semantic* vocabulary pays in a language to which it is added.

To be entitled to claim that there actually are expressions in natural languages which should be understood as playing the anaphoric role just abstractly described and to see what indirect descriptions have to do with specifically *semantic* vocabulary, we must look at some examples. Consider a discourse in which Joe N. says:

(9) #I should have known better than to let the mechanic Binkley work on my car. That airhead misadjusted the valves . . .#

Suppose that, later, Jim, forgetting the name Joe used, says:

(10) #For car repair, don't go to the mechanic Joe N. referred to as "that airhead."#

How are we to understand this latter remark, and in particular the singular term

(11) the mechanic Joe N. referred to as "that airhead" ?

Clearly this term refers to Binkley, Joe N.'s hapless mechanic. But how is this reference secured? The most obvious way to interpret such a singular term is as a straightforward definite description, by analogy to 'the mechanic who worked on Joe N.'s car and misadjusted the valves'. In both cases some purportedly unique feature of Binkley is used to single him out—his relation to Joe N. either in

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being referred to by him in a certain way or in having abused his car in a certain way.

But the anaphoric category of complex cotypically nonintersubstitutable dependents and the brief discussion of anaphorically indirect definite descriptions suggest that an alternative analysis may be more illuminating. For we can think of (11) as being an anaphoric dependent, having Joe N.'s original token of 'that airhead' as its anaphoric antecedent. If tokens of (11) are anaphorically dependent on the original token of 'that airhead', then they are coreferential with it and, hence, refer to Binkley the mechanic. On this account, (11) should be thought of as referring to Binkley in the way a token of 'he' would, if Jim could arrange to ensure that the antecedent of that token of 'he' were Joe N.'s token 'that airhead'. Pronouns, as simple cotypically nonintersubstitutable anaphoric dependents, can take such antecedents if the antecedent and dependent tokens are sufficiently close to each other in time, space, or audience attention. But for distant antecedents, one may not simply rely on the meager resources grammar gives us to work backward from a simple dependent token such as 'he', which even with contextual supplementation can give us only so much information about its antecedent. Here, according to the current suggestion, is where indirect definite descriptions enter. For these locutions are grammatically complex, like ordinary definite descriptions, and enable the use of the full descriptive resources of the language to specify the antecedent token to which they are anaphorically linked.

In our example the antecedent token is specified as that token whereby Joe N. referred to someone as "that airhead," the token directly picked out by the phrase "Joe N.'s utterance of 'that airhead'." Knowing what the individual was referred to *as* tells us the *type* of the antecedent token. Invoking Joe N. locates the particular token of that type which is in question. The presence of 'refer' marks the indirect-addressing feature, by which we are to understand the referent of the whole description to be not the term token picked out as anaphoric antecedent, but rather, as with simple pronouns, the referent of that antecedent token. Indirect definite descriptions like (11) should be understood as complex pronouns (anaphoric dependents), and 'refers' and its cognates should be understood as complex anaphoric pronoun-forming operators.[7]

[7] One difference that might be remarked between ordinary pronouns and those formed by indirect description concerns backwards anaphora, in which the anaphorically dependent occurrence precedes its antecedent in the discourse. Such cases are unusual, though by no means always deviant or strained, for ordinary depend-

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The 'refer' cognates consist of all the sorts of expressions that we would normally think of as being used to assert semantic word-world relations. Thus the following examples ought to be understood according to the model of indirect or anaphoric descriptions: 'the philosopher N. mentioned yesterday', 'the restaurant he talked about at the committee meeting', 'the difficulty discussed above', 'the person denoted by the second name on the list', 'the criminal described by the police in the morning paper', 'the referent of the expression you are pointing to', and "the advisor Kissinger characterized publicly as 'almost a third-rate intellect'."[8]

III

Although there are important differences among these examples, all of them could be paraphrased so as explicitly to use some form of 'refer'. But even this rough characterization is only of use insofar as it is possible to say what is special about the functioning of 'refer' which would enable one, for instance, to tell whether some alien language possessed an expression playing an analogous role. Putting the question more generally, even if it turns out that we can properly account for the behavior of expressions like those in our examples according to the indirect-addressing model of anaphoric descriptions, how could we explain and justify enforcing such a radical distinction between the analyses of descriptions as apparently analogous as (11) and

(12) the one Joe N. startled (insulted, deafened) by his remark about airheads ?

Doesn't the most intuitive reading of (11) assimilate it to (12), treating both as ordinary definite descriptions of a man, who in each case happens to be picked out by his relation to some utterance by Joe N.? What difference between these cases *makes* the difference in virtue of which (11) should be treated as an indirect description

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ents like 'he' and 'the man'. Indirect descriptions, in virtue of the explicit way they pick out the tokens they depend upon anaphorically, exhibit no such prejudice for the discursive past and, accordingly, often possess "antecedents" only in the broader sense of anaphorically inheriting content from another tokening.

[8] This last sort of example shows that the term token can be picked out in a variety of ways, in particular, by citing a predicate type to pick out a sentence token which is the characterizing and which contains the term token (perhaps a tokening of 'Richard Allen') which on the present account is the anaphoric antecedent of the indirect description in the example. We may also note that *in*definite descriptions may similarly be constructed from indirect anaphoric sortals, as in "A woman the lawyer referred to as 'she who must be obeyed' explained the matter to us," which both initiates a new chain *and* characterizes the referent of that chain by anaphoric relation to some antecedent tokening of 'she who must be obeyed' by the lawyer in question. See the discussion of referential predication below.

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that essentially involves an anaphoric link, whereas (12) should be treated as an ordinary description using a relation to an utterance to pick out an object? What is the crucial difference between being referred to by a certain token and being startled, insulted, or deafened by it?

The clearest manifestation of the difference in question concerns the *iteration* of pronoun-forming operators. Because the relation ". . . is an anaphoric dependent of . . ." is transitive, any operator that takes a term token and produces an expression that anaphorically depends upon it should be iterable without change of resulting reference. For tokens of the complex pronoun formed by applying the indirectness operator to (a token of) the result of applying that operator to an original initiating token should simply continue the anaphoric chain—dependents of dependents having the same original or ancestral antecedent.

Consider such iteration as applied to our examples (11) and (12) above. If we iterate the description-forming operators that produced these, with suitable variation of speakers, we get:

(13) the one John M. referred to as "the one Joe N. referred to as 'that airhead'"

(14) the one John M. startled by his remark about the one Joe N. startled by his remark about airheads

If descriptions formed in the appropriate way from 'refers' are indirect descriptions, then, in virtue of the transparent iterability of anaphoric dependence, we should expect (13) to be coreferential with (intersubstitutable for) (11) and, hence, with Joe N.'s original token of type ⟨'that airhead'⟩. And so they are, issues of speaker's reference aside.[9]

But though (11) and (13) are coreferential *de jure*, the superficially analogous (12) and (14) would be coreferential only by accident and under special circumstances.[10] These considerations can be formulated as the *iteration condition* (15) below, which is a necessary condition for understanding an operator PF as a pronoun-

[9] Speaker's reference will be discussed briefly below as an anaphoric phenomenon.

[10] It may seem that the presence of a sortal restriction on indirect descriptions causes difficulties. The issue can be avoided as in (11) above, however, by using an *anaphorically dependent* sortal. 'One' anaphora has long been recognized by linguists as permitting anaphoric proforms as stand-ins for common nouns, as in "There were red pens as well as green ones on the table." In fact philosophers have made up the expression 'referent of *t*' to mean "the one referred to by *t*." So the fact that indirect descriptions are sortally restricted, as are ordinary descriptions or quantifications in natural language, adds no new difficulties to an anaphoric analysis of 'refers'.

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forming operator. If we agree to surround a term designation with angle brackets to form a designation ⟨term⟩ of the *type* of that term, and to surround such a type designation with subscripted slashes to form a designation /⟨term⟩/$_i$ of a token of that type, then the iteration condition can be put as follows:

(15) $\text{PF}(/\langle\text{term}\rangle/_i) = \text{PF}(/\langle\text{PF}(/\langle\text{term}\rangle/_i)\rangle/_j)$

It is clear that nothing can correctly be thought of as a pronoun-forming operator unless it meets this condition embodying the transitivity of anaphora. Our strategy will be to exploit such an iterability requirement, suitably qualified, as a *sufficient* condition for identifying operators that form indirect descriptions (which have been explained as type-substitution invariant, lexically complex pronouns).

Enough weight will be placed upon the strategy of transforming the iterability condition from a necessary to a sufficient condition for interpreting a syntactically relational expression as a complex pronoun-forming operator to make it worth stating precisely. The question the test is supposed to answer is this. Suppose we are given a construction that on the surface has the form

(16) the x[REF(x,/⟨term⟩/$_i$)]

The over-all expression appears to be a definite description that picks out a thing x by means of its relation REF to a token /⟨term⟩/$_i$ of type ⟨term⟩. An example would be

(17) the man who was frightened when Bernadette uttered a token of type 'BOO'

The iterability test tells us to consider (16) and

(18) the y[REF(y,/⟨the x[REF(x,/⟨term⟩/$_i$)]⟩/$_j$)]

as in

(19) the man who was frightened when Bernadette uttered a token of type "the man who was frightened when Bernadette uttered a token of type 'BOO'"

and (13) and (14).

The claim is that the syntactically relational expression REF here should be understood *not* as standing for a relation, as in an ordinary definite description, but as an anaphoric operator forming in-

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direct descriptions, if and only if the following three conditions are met[11]:

(20) *If* (16) is a proper description (that is, in fact picks out one and only one object) and if (18) is a proper description, *then* they corefer (are intersubstitutable).

(21) *If* the token by relation to which the individual in (18) is picked out *were not* of the same type as the expression (16), *then* (16) and (18) would *not* in general corefer.[12]

(22) Accepting the identity statement by means of which the coreference of (16) and (18) is asserted is not accidental, in the sense that it is a condition of being taken to understand the expressions involved.

The first condition is required because expressions of type (18) need not always pick out unique objects even when the expression of type (16) does. There need be no one who is the man Bernadette frightened by uttering a token of type "the man Bernadette frightened by uttering a token of type 'BOO'", and similarly for genuinely anaphoric cases. Iterability is a relevant test only in the (in general counterfactual) situations where the appropriate individuals exist. The second condition is required in order to rule out cases where the same individual is picked out no matter what token one looks at—the case where one and only one man is frightened by *whatever* Bernadette says. The type of the intermediate antecedent of an anaphorically dependent expression is obviously essential to its having the reference that it has; so this condition represents a natural constraint. The third condition is required in order to rule out grammatically accidental coreference of (16) and (18), as might happen in a psychologically homogeneous population with the relation "is the first object one is reminded of on hearing the expression ⟨term⟩".[13] Together these three conditions ensure that any expression 'REF' which satisfies them may appropriately be understood as forming expressions of type (16) which ought to be under-

[11] The identification of indirect descriptions by the iteration test provides us with a sense in which one term *type* can be anaphorically dependent on another type, as the iterated indirect description is on the type of its antecedent token. But this notion of type-anaphora is entirely derivative from the basic notion of token-anaphora, a derivation made possible by the existence of operators that form lexically complex, type-substitution-invariant pronouns.

[12] It may seem that talk here of "picking out", "coreferring", and so on begs important questions. But it will be shown below that such talk can be understood in a way that does not commit one to reference relations.

[13] Joe Camp pointed out the need to deal with these relations which "piggy-back" psychological relations on semantic ones.

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stood as really having the form of complex anaphoric dependents, that is, as indirect definite descriptions like (11) rather than as ordinary direct descriptions like (12). Frege placed great theoretical weight on the intersubstitutability of the terms '*t*' and "the Bedeutung (or referent) of '*t*' ",[14] and this same essential feature of referring lies at the center of the present account.

IV

The account so far has described the anaphoric category of indirect descriptions as a form of pronoun, has offered a formal test discriminating operators that generate expression types of this category, and has pointed out that 'refers' as it appears in contexts like

(23) the one Kissinger referred to as "almost a third-rate intellect"

can be understood as such a complex pronoun-forming operator. But there are other important uses of 'refers' and its cognates. The most fundamental of these are: "Tarskian" contexts, such as

(24) 'Rabbits' refers to (denotes) rabbits.

denials of reference, such as

(25) (The expression) 'the present King of France' does not refer (or refers to no one).

mere reference claims, as in

(26) During his talk the speaker referred to Napoleon.

and referential predications, as in

(27) The speaker talked about shadowy figures from the intelligence community.

The strategy is to approach these locutions in two stages. First, each such usage is paraphrased into a form in which the 'refers' cognate appears *only* inside an indirect description. Then that description is explained as functioning as a complex pronoun, according to the story already told. The present concern is thus with the paraphrase in terms of indirect descriptions.

The generalization of the token-based account of indirect descriptions required for Tarskian contexts is really a simplification to a special case. For statements about what a term refers to or denotes presuppose that the term type in question is type-substitution in-

[14] See the discussion in the author's "Frege's Technical Concepts," forthcoming in *Synthese*.

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variant—that all cotypical tokens corefer. So we can parse

(28) The term 'Leibniz' denotes Leibniz.

as an identity involving an indirect description[15]:

(29) The one denoted by the term 'Leibniz' is (=) Leibniz.

Given the presupposition of type-substitution invariance of the type 'Leibniz' in the original claim, this indirect description is equivalent to

(30) the one denoted by any token of 'Leibniz'

which we may then straightforwardly understand as a complex pronoun, anaphorically dependent upon an antecedent which may be any token of the specified type, e.g., that which appears on the other side of the identity sign. The felt triviality of such reference claims is thus explained.[16] Of course, type-substitution-invariant reference claims need not be epistemically trivial if different languages or different term types are involved, as in

(31) In our world (the expression) 'the first Postmaster General' refers to Benjamin Franklin [or: the inventor of bifocals].

which we understand as using anaphoric relations to claim

(32) In our world, the one referred to as "the first Postmaster General" is (=) Benjamin Franklin.

which is not a trivial assertion.

Such an account respects the different modal status of (28), which is contingently true, and "Leibniz is Leibniz", which is necessarily true. For the possibility that (28) is not true can be understood in terms of the paraphrase (29) as the existence of a possible world w such that the one referred to as 'Leibniz' in w is not Leibniz (that is, the one *we* refer to in our own world as "Leibniz"). The explicit relativization of the indirect description to a possible world w simply tells us which world its antecedent tokens are to be found in. The candidate antecedents of "the one referred to as 'Leibniz' in w" are tokenings of 'Leibniz' uttered in w. The anaph-

[15] Compare Frege's similarly motivated paraphrase of "Jupiter has four moons" into "The number of Jupiter's moons is (=) four" in section 57 of the *Grundlagen*.

[16] If one sets up Tarskian truth conditions using denotation claims for semantic categories besides terms, these can be accommodated as well by this scheme (although it is no part of the present project to discuss other parts of speech). Here one thinks of reading "'Red' refers to (denotes) red things" as "the ones referred to as (denoted by) 'red' are red things", and "'magnetic' applies to (has in its extension) magnetic things" as "the ones 'magnetic' applies to are magnetic things".

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oric approach thus has room for what has been thought of as the contingency of word-world semantic relations, although it does not appeal to such relations.

*Co*reference claims represent a simple variation on Tarskian contexts, and can be interpreted in much the same way. Thus to say that the expressions ⟨type$_1$⟩ and ⟨type$_2$⟩ corefer is just to assert an identity between the corresponding indirect definite descriptions, i.e., to say that the one referred to by ⟨type$_1$⟩ *is* the one referred to by ⟨type$_2$⟩. Asserting such an identity is licensing as commitment—preserving the intersubstitution of expressions of those types. In the case of cotypically nonintersubstitutable expressions such as demonstratives, asserting an identity authorizes substitution of anaphoric dependents of the token on the left for anaphoric dependents of the token on the right of the identity, and vice versa.

It may be worth noticing that, if attention is restricted to type-substitution-invariant term types, the iteration test introduced above can be simplified correspondingly, by omission of token specifications. The necessary condition for operators PF to form complex type-substitution-invariant pronouns is then:

(33) $\text{PF}(\langle \text{PF}(\langle \text{type} \rangle) \rangle) = \text{PF}(\langle \text{type} \rangle)$

and the corresponding condition suggested as sufficient for REF to be an indirect description-forming operator is

(34) the $y[\text{REF}(y, \langle \text{the } x[\text{REF}(x, \langle \text{type} \rangle)] \rangle)] = \text{the } x[\text{REF}(x, \langle \text{type} \rangle)]$

Simple negations of statements of reference, as in

(35) (The expression) 'the shortest man in the room' does not refer to John.

raise no new issues, for we have already explained the underlying identity that is being negated. But claims such as (25), that an expression does not refer to *anything,* deserve special mention. The obvious way of extending to these cases the previous strategy of paraphrasing what look like assertions of reference relations as identities involving indirect descriptions is to quantify into the identity and read the result as a negative existential statement. That is, statements of the form

(36) ⟨type⟩ does not refer.

are to be read as

(37) The one referred to as ⟨type⟩ does not exist.

where this last is to be understood just as we understand negative

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existentials involving ordinary direct descriptions, as in

(38) The present King of France does not exist.

That the indirect definite descriptions involved in denials of referentiality are anaphoric dependents makes no difference to our reading of the negative existentials; we have no trouble understanding remarks like

(39) I would be comforted by the benevolence of a supreme being, except that such a being does not exist.

in which 'such a being' is an anaphoric dependent.

Statements like (26) tell us that reference has taken place, but give no information about what the referring tokens or types were. Such remarks may be understood as asserting that there is *some* term tokening *t* in the speaker's discourse such that the item referred to by *t* is Napoleon. Statements like (27) are similar, except that a predication, rather than an identity, is what is asserted of the items referred to or talked about. (27) says that there were tokenings *t* and *t'* and perhaps more, such that the items referred to (or talked about) by *t* and *t'* have the property of being shadowy figures from the intelligence community. These are predications involving pronouns, intrinsically no more mysterious than sentences like "They are confused." Common nouns can be formed from indirect descriptions just as they can be from ordinary direct descriptions, and the present account extends straightforwardly to these expressions, as in

(40) All the animals the speaker mentioned tonight were quadrupeds.

This example indicates as well how *generalizations* about reference are to be approached anaphorically. Endorsement of (40) commits one to all the substitution instances of the form "If *t* is an animal the speaker mentioned tonight, then *t* is a quadruped." The antecedent of each such conditional is a referential predication, equivalent to "There is a term token */s/* such that the speaker uttered */s/* tonight and the item referred to by */s/* is an animal and the one referred to by */s/* is (=) *t*," which we already know how to interpret. As long as we can in this way understand each of the substitution instances to which a universally quantified claim commits us, we can understand the universal generalization itself. A full discussion of such cases would require an account of anaphora and quantification which goes beyond the scope of this essay. The complications arise in part from the recognition that in the general case the term substituend *t* which is repeated in the specifi-

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cation above of the form of each sentential substitution instance of the quantification would need to be replaced by two term tokens, one of which is anaphorically dependent upon the other. The present point is that, although an account is not being offered of quantification in general, it is clear from the example that no new difficulties are added by the presence of indirect descriptions in the quantificational substitution instances, so that generalizations about reference can be understood if any sort of generalizations can.

Consideration of generalizations about what is referred to by various expressions makes salient another issue, which can only be dealt with in passing here, namely the susceptibility of an anaphoric account of reference claims, to the formulation of semantic paradoxes. In the presumably analogous case of truth, a naive substitutional understanding of quantification into truth claims commits one to interpreting paradoxical sentences such as the Liar. Of course generalization is not the only way in which such paradoxical expressions can arise, nor is the possibility of semantic paradox restricted to the category of sentences. It is possible to use 'refers' to formulate empirically paradoxical term tokens such as

(41) the square root of 2 which is the result of multiplying −1 by the one referred to by the term token numbered '41'

where 'square root of 2' is a sortal comprising the positive and negative roots and 'one' is understood as a prosortal anaphorically dependent upon it. Interpreting such tokens as anaphorically indirect descriptions focuses attention on grounding conditions for anaphoric inheritance—a large and important topic. In "Inheritors and Paradox" Dorothy Grover[17] elaborates an anaphoric approach to semantic paradoxes for the closely analogous anaphoric treatment of '. . . is true.' Grover finds that the natural condition on anaphoric grounding yields an interpretation coinciding in general with the sentences that Saul Kripke assigns a semantic value to at the minimal fixed point, the interpretation he takes to provide the most natural model for the intuitive concept of truth. Her remarks can be applied to the present construction by means of the crucial

[17] This JOURNAL, LXXIV, 10 (October 1977): 590–604. Also in this paper Grover independently states a weaker version of the observation which is exploited in the present account: "Descriptive phrases such as 'just mentioned', 'been talking about', 'are referring to', which ostensibly describe discourse may often be used merely to locate an antecedent piece of discourse from which a referent is inherited" (594). See also Grover's "'This Is False' on the Prosentential Theory", *Analysis*, XXX.2, 170 (January 1976): 80–83.

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analogy between this pronominal account of reference and the prosentential account of truth which inspires it.

V

The task of this paper has been to make it plausible that the use of 'refers' and cognate locutions in natural languages can be understood by first paraphrasing contexts in which they occur into a form in which they appear only inside indirect descriptions and then understanding their role in those paraphrases as operators taking token (or type) specifications and forming from them lexically complex, type-substitution-invariant pronouns whose anaphoric antecedents are the specified tokens. As just indicated, such an account of 'refers' should be compared to the prosentential account of 'true' due to Grover, Joseph Camp, and Nuel Belnap.[18] For, following the lead of that account, '. . . is true' can be seen as an operator that forms pro*sentences*.

The prosentential approach points to two uses of pronouns, lazy uses in which the pronouns can be replaced without loss of content by (a transform of) their antecedents as in (4), and quantificational uses in which their antecedents determine a *class* of admissible substituends, as in "If any object gravitates, then it has mass." So too pro*sentences*, which, by analogy to indirect descriptions, we can understand as formed by using '. . . is true', have both lazy and quantificational uses. If I say "Snow is white," and you say "That is true," your remark would have had just the same content if you had simply repeated mine, save for the disclaimer of originality always involved in anaphora. It is for these "lazy prosentences" that F. P. Ramsey's redundancy approach to truth was developed. "Everything the policeman said is true," can be rephrased as "If the policeman said it, then it is true," and 'it is true' may then be understood in turn as functioning as a prosentence of quantification, by analogy to the second role of pronouns. The innovation in the prosentential account is to see the entire expression 'that is true' or 'it is true' as an anaphoric dependent in the grammatical category of sentences, instead of (or as well as) seeing 'that' and 'it' as anaphoric dependents in the grammatical category of singular terms. In this way, it is argued, the uses of '. . . is true' in natural languages can be accounted for.

[18] Grover, Joseph L. Camp, and Nuel D. Belnap, "A Prosentential Theory of Truth," *Philosophical Studies*, XXVII, 2 (February 1975): 73–125. The ensuing discussion presupposes the exacter analogy made possible by revising the prosentential theory so as to see ". . . is true" as a prosentence-forming operator rather than as a syncategorematic fragment of an indissoluble prosentence such as 'that is true'. I elaborate such a revision in "Sentential Anaphora and Truth", ms.

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The present theory of 'refers' simply does for terms what the prosentential theory did for sentences (although we have not discussed quantification). Each theory explains the use of a bit of *semantic* vocabulary in terms of the formation of *anaphoric* proforms. The uses of the sentences by means of which we meant to be making claims about what objects in the world our talk refers to, and what it is in the world in virtue of which what we say is or is not true, are explained in terms of the purely *intra*linguistic relation of anaphora.[19]

Indirect descriptions formed from 'refer' *both* mention a term expression (in picking out anaphoric antecedents) *and* use that expression. The effect of applying an indirect description-forming operator to a mentioned term is that of *turning* the mentioned occurrence into a used occurrence. Thought of in this way, 'refers' is an *anaphoric disquotation operator* in the same sense that 'true' is.[20] Further argument would be required to maintain that the reference claims so formed deserve to play a basic explanatory role in giving an account of the *use* of term expressions. For it would seem that disquotation operators simply presuppose the features of the use of unquoted expressions which the linguistic theorist takes as explanatory target.

There is one potentially significant sort of locution which does not receive an analysis on this approach. For although accounts are offered of what someone referred to by an utterance and of what the reference of the utterance was, nothing is said about the *relation* of reference. The anaphoric approach will not tell us how to understand sentences such as

(42) Reference is a physical, causal relation.

The reason is clear. On the anaphoric account although '. . . refers to---' plays a syntactically relational role, its semantic role is anaphoric and pronominal rather than relational. Philosophers have misconstrued the plain man's use of 'refers' and hypostatized a relation of reference as the semantic interpretant of the apparently rela-

[19] As an example of the sort of philosophical question raised but not answered by the possibility of such an anaphoric account of semantic vocabulary, consider the distinction between substitutional quantification and the referential or objectual variety. Although the technical differences are clear between reading a quantifier in terms of an algebraic model via an interpretation function and by looking at linguistic substituends, the standard Quinean account of what turns philosophically on this technical difference seems to be undercut by the possibility of an anaphoric understanding of the interpretation statements involving 'denotes' or 'refers'.

[20] The specially anaphoric nature of the disquotation involved is most apparent in cases where the indirect description picks up the speaker's reference of its mentioned antecedent.

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tional expressions, and have then asked for a theory of it. Such a mistake is of a piece with the search for the objects corresponding to each expression that syntactically plays the role of a term—e.g., 'someone' and 'everyone'. Our ordinary remarks about what some individual or expression refers to or has as referent are perfectly in order, and the anaphoric account tells us how to understand them. But reference is a philosopher's reification and a fiction, generated by a grammatical misunderstanding. The present account distinguishes sharply between expressions and their referents (what is referred to by them), in that very different uses are associated with the expressions "the expression 'Leibniz'" and "the referent of the expression 'Leibniz'." No doubt, as with any other two items in the causal order, there are many relations that can correctly be said to obtain between a term token as it stands and what it refers to. But the present considerations show that our talk about referring and referents gives us no reason whatsoever to conclude that some one of these could be singled out as *the reference relation,* that unique semantically significant word-world relation in virtue of which the non-expression is the referent of the expression. Various word-world relations may play important explanatory roles in philosophical projects, but to think of any one of these as what is referred to as "the reference relation" is to be bewitched by surface syntactic form.

In order to see what is and what is not being claimed for this analysis of 'refers' and to see the significance of the replacement of a relational account of reference with an anaphorically disquotational account of referring, it will help to redescribe and clarify the explanatory role that the concept of indirect descriptions is supposed to play in that analysis. Consider first a language-in-use such as English, and produce its nonsemantic fragment by extruding from it all the sentences containing semantic vocabulary ('refers' and its cognates for the present account, and 'true' as well if we appeal to a prosentential theory). Suppose that a theorist has succeeded in capturing the social practices governing the use of this *non*semantic fragment, at least in the sense that the theory attributes to each interlocutor a stock of sentences that this interlocutor is prepared or committed to assert and a set of inferences he is bound to endorse, and describes how assertion and inference proceed given these attributions.[21] Then the anaphoric account of 'refers' developed here permits the extension of such an account of the

[21] See for instance my "Asserting" *Noûs,* XVII, 4 (November 1983): 637-650.

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use of the *non*semantic fragment of the language to an account of the use of the whole language.

First, an artificial operator REF stipulated to meet the iteration test can be introduced, by means of which new singular terms can be formed on the model of (16). These new terms can then be understood, following the discussion of anaphoric categories, as indirect descriptions. Understanding them in this way determines their role in assertion and inference. For one is committed to endorsing "the one referred to by $\langle term_1 \rangle$ is $\langle term_2 \rangle$" just in case one is already committed to the nonsemantic substitution-licensing identity "$\langle term_1 \rangle = \langle term_2 \rangle$,"[22] and similarly for referential predications. Next, using the paraphrase strategy outlined earlier, the sentences of the semantic fragment of the original language can be translated into the sentences of this artificial extension of its *non*semantic part. The claim is then that such a translation will correctly predict the inferential endorsements and assertional commitments in the semantic fragment, on the basis of only those endorsements and commitments involving *non*semantic vocabulary. In this sense the semantic fragment of the language is reduced to its nonsemantic base by the anaphoric analysis.

It is at this point that a theorist seeking a better account of the pragmatic dimension of his base language might introduce a second artificial operator SP REF expressing speaker's reference. Such an operator would be defined in terms of the basic semantic/anaphoric notion of reference as captured by REF. Although SP REF would not be required to pass the iteration test [the equivalence of (13) to (11) clearly breaks down if we let 'refers' include speaker's reference], the use of indirect descriptions such as "the one Joe speaker-referred to as 'that fat politician'" will still be determined by the use of the nonsemantic base language, in virtue of an account of SP REF in terms of REF. The idea is that it is incoherent to take someone as having speaker-referred to an object unless one also takes it that that individual *could have* referred to it "semantically." Had I known there might be a question about whether there was champagne or ginger ale in the glass, I would have used the description "the man in the corner with bubbly liquid in his glass" instead of a more daring description. And it is these safe or minimal descriptions standing behind speaker's references which should be seen as the ultimate anaphoric antecedents of indirect descriptions such as "the one speaker-referred to as 'the man in the corner with champagne in his glass'", which themselves depend

[22] Type-token niceties are brushed over here.

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upon such covert or merely virtual tokenings [just as 'that airhead' anaphorically depends upon Joe's tokening of 'the mechanic Binkley' in (9), and so would refer to Binkley even if he were not in fact an airhead]. So parsing indirect descriptions formed using SP REF just requires an extra step in determining the anaphoric antecedent, but otherwise raises no difficulties for the translation scheme into artificially extended nonsemantic English.

Suppose that semantic talk can in this way be reduced to nonsemantic talk by anaphoric links between the two kinds of talk secured by indirect descriptions, without invoking a relation of reference (or a property of truth). It might be thought that all such an intralinguistic reduction establishes is that *if* one can give an account of the use of the nonsemantic portion of the language without invoking a reference relation, *then* one can extend it to an account of semantic talk about referring without needing to *add* a notion of reference to account for our apparent talk about such a relation. The possibility may then seem open that a notion of reference is still required to account for the use of the nonsemantic fragment appealed to by disquotation. Such a response would be a mistake. For what is envisaged is that, in order to explain how nonsemantic sentences of the object language are used, the linguistic theorist must make claims in the metalanguage which specify what object-language expressions refer to. But the anaphoric account will apply equally to metalanguage statements of the form

(43) ⟨term⟩ in OL refers to electrons (ML).

No reference relation need be invoked to understand the role of these metalanguage semantic claims any more than for the semantic fragment of the object language. Understood by their paraphrases in terms of indirect descriptions, claims like (43) add the expressive capacity to pick up object-language antecedents with metalanguage anaphoric dependents, and so permit the absorption of the language under investigation into the theorist's use-language. The effect of adding to ML expressions of the form *the one referred to in OL by* ⟨*term*⟩ is to permit the expression in ML of identities and predications involving OL expressions, including cotypically nonintersubstitutable ones or those used to speaker-refer.

Has reference been explained away? There are four reasons for which one might think that it has not. First, it might appear that an intralinguistic account of referring is committed to a sort of linguistic idealism which cuts language off from the world. But the present account distinguishes, e.g., the expression 'Leibniz' from the one referred to by that expression, and does not treat the latter

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as in the linguistic order. Expressions do refer to nonlinguistic items (it is true that the one referred to by ⟨Leibniz⟩ is not a word and was a person). It is claimed only that it is a mistake to think that there is some relation of reference being invoked when we say this. Second, one might worry that reference had been smuggled into the account of indirect descriptions. For that account required that the antecedent token be described, picked out, or otherwise referred to before an indirectness operator is applied to produce a pronoun dependent on that antecedent token. Even though the things referred to are always term tokenings and hence in the linguistic order as well as in the causal order, haven't referential relations been presupposed? They have not. For the anaphoric account tells us just how to understand claims about *what is referred to as "Joe's utterance yesterday of the term 'that airhead' "*. The starred expression is just another indirect description. This response points to the third worry, and to a genuine qualification. For the notion of anaphoric dependence upon which the whole account rests has not been explained, but only introduced by example. Unless anaphora can be explained without invoking a relation of reference, the attempt to use it to explain away reference will be circular. I believe that such an account can be given in terms of inheritance by the dependent token of identificatory and, hence, substitution-inferential commitment from the antecedent token; but justifying such a claim is outside the scope of this essay. So reference is explained away only taking for granted a notion of anaphora.

The final reason one might have for denying that the dispensability of a relation of reference has been established by the foregoing considerations is sociological. If there is no relation of reference, surely some account is required of what has misled us into looking for such a relation as the core of an explanation of linguistic practice. Put another way, the story told here about the expressive function of indirect descriptions (and hence of claims about what refers to what) seems to preclude their use in *explanations* of nonsemantic linguistic practice, on grounds of circularity. For, understood anaphorically, metalanguage reference claims will presuppose the object-language uses which are the antecedents of those metalanguage indirect descriptions and, hence, cannot be used to explain those uses. Yet it is just to achieve such explanations of the nonsemantic practices of the language that theorists have always cared about the semantic relations here explained away anaphorically. It is in this context that the anaphoric account presents us with a closing dilemma. *Either* it must be possible to

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offer an account of the project of explicitly codifying in a theory the assertional and inferential practices that constitute the use of a language in such a way that it becomes clear why the special expressive resources of anaphoric proform-forming operators in the theorist's language are crucial to that project, *or* it must be possible to explain what it is about the functioning of this anaphoric vocabulary which has *mis*led theorists into attempting to use that vocabulary to express the quite different notions that really *are* crucial to that explanatory project.

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THE PLAUSIBILITY OF RATIONALISM*

THE rationalist-empiricist debate that preoccupied so many linguists, philosophers, and psychologists during the late sixties ran out of steam in the mid-seventies when both parties eventually tired of unsuccessful attempts to convert each other. In this paper I wish to reopen the discussion of issues raised in that debate, because I believe that we are now able to formulate these issues with sufficient clarity to be able to assess the plausibility of rationalism. Specifically, I believe that we can now provide strong arguments for the plausibility of the so-called "innateness hypothesis." Such vindication seems especially timely, since there is a growing number of psycholinguists who believe that according semantics a substantial role in syntax acquisition makes unnecessary the rationalist assumption that the learner has rich innate knowledge about the class of possible natural languages. In defending the innateness hypothesis, I distinguish the commitment to rich innate structure from the concomitant rationalist commitment to an intentional and, more specifically, propositional-attitude account of language acquisition. It is, I argue, an open empirical question whether such accounts of language acquisition can satisfy the usual criteria for explanatory adequacy; the correct account may be neither empiricist nor rationalist. I begin by stating what seems to me to be at issue between rationalists and empiricists.

* An earlier version of this paper was presented at the annual meeting of the Society for Philosophy and Psychology, March, 1980. In revising the paper I have benefited from the criticisms and suggestions offered by several people, especially Ned Block, Noam Chomsky, William Demopoulos, Robert May, and Stephen Stich.

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I. THE RATIONALIST-EMPIRICIST DEBATE

The contemporary rationalist–empiricist debate, like its seventeenth-century antecedent, focuses on learning, specifically on the respective contributions of experience and innate structure of mind. The basic issue is this: What sort of innate structure must be postulated in order to explain how, on the basis of the experience we have, we come to know what we know? The contemporary debate differs from its antecedents in choosing to pose the question in terms of the acquisition of language rather than in terms of the acquisition of concepts or ideas. The current interest in language acquisition arises not so much because rationalists have typically been committed to the species-specificity of linguistic capacity, but rather because contemporary rationalists believe that language acquisition provides an appropriate domain for joining the debate with empiricists, since this is the one domain for which we have a tolerably clear idea of what is learned and, hence, of what must be accounted for.

The innateness issue separating rationalists and empiricists is not, as some believe, that empiricists fail to credit the mind with any innate structure. Rather it is that the innate structure hypothesized by empiricists is not thought to place any substantive restrictions on the form of knowledge that can be acquired. As Edward Sapir has put it, language can vary "without assignable limit." The abstract model hypothesized by empiricists to explain syntax acquisition is a device consisting of certain elementary peripheral processing mechanisms that segment the incoming acoustical signal into strings and certain elementary inductive procedures that construct generalizations over those strings.[1] The induced generalizations constitute a grammar for the acquired language inasmuch as they specify well-formedness conditions for strings of that language. The inductive procedures incorporated in the empiricist model are thought to place no substantive restrictions on the class of grammars that can be acquired. In principle, the peripheral processing mechanisms could place quite severe restrictions on what can be acquired, but because these same mechanisms are thought to be implicated in a variety of nonlinguistic perceptual processes, empiricists assume quite reasonably that these mechanisms are of a nonrestrictive, elementary sort. As a consequence, the empiricist language-acquisition device is, in theory, a very powerful general learning device: memory and real-time effi-

[1] Cf. Jerrold J. Katz, *The Philosophy of Language* (New York: Harper & Row, 1966), pp. 240–242.

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ciency limitations aside, it is capable of acquiring any well-defined (i.e., finitely specifiable) language.

The rationalist language-acquisition device, by contrast, is assumed to be severely restricted in the class of grammars it can acquire on the basis of access to data available to learners. For, in addition to the peripheral processing mechanisms hypothesized by empiricists, there are assumed to be innate principles that determine the form of the acquired knowledge in a highly restricted and organized way. This assumption has been dubbed the "innateness hypothesis" by its critics.[2] Much misunderstanding of the rationalist position stems from a failure to understand what claims are (and are not) expressed in the innateness hypothesis; so, before turning to a consideration of the arguments supporting the hypothesis, I wish to make clear what I take to be the *two* claims constitutive of it. First, there are claimed to be severe restrictions on the class of grammars that can be acquired on the basis of the given access to data. Second, these restrictions are thought to be levied by certain innate constraints that determine the form of the grammars that can be acquired. The innateness hypothesis does not make any claim regarding the existence of innate *ideas* or *knowledge* (what for lack of a better word I shall refer to as innate "content"), though it is certainly a rationalist tenet that what is innate is content as well as capacity or mechanisms of learning. Nor does the hypothesis claim either that human linguistic capacity is species-specific or that it is task-specific, though most rationalists endorse both claims for reasons having to do with the innateness hypothesis. Finally, the hypothesis does not provide an account of language acquisition; rather it hypothesizes that an adequate account will satisfy the two claims or conditions stated above.

If one reads the various criticisms of rationalism, one might suppose that there are a number of different arguments advanced by rationalists in support of the innateness hypothesis. Some of these arguments apparently emphasize the ease or rapidity with which language is acquired, others the existence of linguistic universals, and still others the absence of conditioning or reinforcement as part of the normal conditions under which language is acquired.[3] In fact there is only one argument. These other "arguments" represent supporting evidence which is meant to

[2] For Chomsky's own formulation of the innateness hypothesis, see his *Reflections on Language* (New York: Pantheon, 1975), pp. 12/3, 33/4.

[3] Cf. Hilary Putnam, "The 'Innateness Hypothesis' and Explanatory Models in Linguistics," *Synthese*, XVII, 1 (March 1967): 12–22.

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strengthen that argument in ways that I shall endeavor to make clear later in the paper. The one argument upon which rationalists depend is the so-called "argument from the poverty of the stimulus." It is put this way by Noam Chomsky:

> Even knowing very little of substance about linguistic universals, we can be quite sure that the possible variety of languages is sharply limited. Gross observations suffice to establish some qualitative conclusions. Thus, it is clear that the language each person acquires is a rich and complex construction hopelessly underdetermined by the fragmentary evidence available. . . . Nevertheless, individuals in a speech community have developed essentially the same language. This fact can be explained only on the assumption that these individuals employ highly restrictive principles that guide the construction of grammar. Furthermore, humans are, obviously, not designed to learn one human language rather than another . . . . Powerful constraints must be operative restricting the variety of languages (*Reflections*, 10/1).

Versions of this argument are to be found in the writings of virtually all contemporary rationalists; yet, surprisingly, very few critics seem to have noticed the argument, much less discussed or understood it. Hilary Putnam, for example, fails to mention the argument in his discussion of the innateness hypothesis. W. V. Quine acknowledges as indisputable Chomsky's claim that we must attribute to the organism, as an innate property, a structure rich enough to account for the fact that the postulated grammar is acquired on the basis of the given conditions of access to data; the behaviorist, says Quine, is "knowingly and cheerfully up to his neck in innate mechanisms of learning readiness."[4] But Quine's endorsement of the rationalist position may be more apparent than real, for it is unclear whether in conceding the existence of innate mechanisms of learning readiness he takes himself to be conceding the existence of innate principles that place substantive restrictions on the class of grammars that can be acquired on the basis of the given conditions of access of data.

Margaret Atherton and Robert Schwartz discuss the poverty-of-the-stimulus argument at some length in their paper "Linguistic Innateness and Its Evidence,"[5] but they dismiss it as empirically unacceptable and explanatorily vacuous. There is, they claim, no evidence presently available that would show that a language generated only by a grammar that violates the hypothesized restrictions

[4] "Linguistics and Philosophy," in Sidney Hook, ed., *Language and Philosophy* (New York: NYU Press, 1969), p. 95.

[5] This JOURNAL, LXXI, 6 (March 28, 1974): 155–168.

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is unlearnable. Noting that animal-learning studies are irrelevant to this construal of the innateness hypothesis (something, incidentally, that rationalists have always maintained), Atherton and Schwartz conclude (161) that "construing the innateness hypothesis as telling us what cannot be learned seems unacceptable on empirical grounds. Such a theory, moreover, really gives us no purchase on an account of how language is acquired." Their second claim here presents a challenge to rationalists, though hardly a refutation of the innateness hypothesis, since, as I pointed out above, the hypothesis does not presume to be a theory of language acquisition. Their first claim is simply a *non sequitur*: the fact that animal studies cannot support the poverty-of-the-stimulus argument does not show that there is no support for the argument, much less that it is unacceptable on empirical grounds.

Atherton and Schwartz's reasons for rejecting the poverty-of-the-stimulus argument are, seemingly, these. First, they do not see how the argument could be supported except by evidence of the sort that they consider and then reject as inconclusive. They are presumably led to believe that animal-learning studies might be relevant because they misconstrue the innateness hypothesis, taking it to be a claim to the effect that certain grammars cannot be learned on the basis of *any* given conditions of access to data. In fact, the hypothesis is a claim to the effect that only a restricted class of grammars can be learned on the basis of the sort of data from which human learners acquire natural languages. Their second reason is this: they realize that the poverty-of-the-stimulus argument can easily be extended to other organisms and to other domains of learning. They follow Nelson Goodman in seeing in this consequence a *reductio* of the poverty-of-the-stimulus argument and hence of the rationalist position. Surely, to use Goodman's own example, learning to identify a painting as by a certain artist or of a certain school or period cannot be thought to require that the mind has been endowed with a "schematism" of artistic styles, even though this ability is acquired on the basis of rather few examples. Atherton and Schwartz locate the supposed flaw in the poverty-of-the-stimulus argument in the notion that language-learning might involve the construction and testing of hypotheses:

> Hypotheses, at least from the standpoint of inductive logic, can themselves occur only as part of some language or system of representation. . . . To make literal sense of an inductive-logic model dealing in evidence and hypotheses, there would have to be another prior symbol system in which to formulate the evidence and hypotheses. The hypotheses and their evidence cannot be formulated in the language

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> being learned, for it is just this learning event that the hypothesis model is supposed to explain. And, of course, even if we assumed that the evidence and hypotheses were available in some prior symbol system, the model as an account of how symbolic skills are learned must break down, since it leads to a regress (163).

Atherton and Schwartz's argument here is not compelling. Even if hypothesis-testing models do commit rationalists to the existence of a language or system of representation in which the hypotheses are explicitly represented (something which I doubt), the envisioned regress need not occur. The indisputable fact that all natural languages are acquired is fully compatible with there being an innate language or system of representation in terms of which the hypotheses and evidence postulated by a hypothesis-testing account of language acquisition might be couched. Some rationalists who endorse such an account have urged that there are independent reasons for hypothesizing such an innate "language of thought."[6]

Although their criticisms of the poverty-of-the-stimulus argument fail, in part because they misconstrue the innateness hypothesis as providing an account of language acquisition, Atherton and Schwartz do raise an important question regarding rationalist accounts of language acquisition: Does the hypothesis-testing idiom in which these accounts are typically cast manifest a further commitment on the part of rationalists to certain assumptions regarding the appropriate theoretical vocabulary for characterizing the internal states and processes of the learner? If so, then the plausibility of rationalism depends not only on the plausibility of the innateness hypothesis, but also on the plausibility of these assumptions about the appropriate theoretical vocabulary. I shall return to this question later in the paper; I wish now to develop in greater detail the poverty-of-the-stimulus sketched above. Specifically, I wish to provide a plausibility argument for the rationalist claim that there are severe restrictions on the class of grammars that can be acquired on the basis of the access to data available to human learners.

II. A PLAUSIBILITY ARGUMENT FOR THE INNATENESS HYPOTHESIS

In acquiring a first language, the learner acquires knowledge of this language on the basis of certain relevant experiences which constitute the *primary data* for the acquisition task. If, as is generally assumed, the primary data and the acquired linguistic knowledge can be characterized formally in terms of a data sequence D_i and a grammar G_j, respectively, then, neglecting any individual

[6] See Jerry Fodor, *The Language of Thought* (New York: Crowell, 1975).

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differences in human linguistic capacity, the learner can be construed as a function on the natural numbers, from indices of data sequences D_i to indices of the grammars G_j that can be acquired on the basis of D_i. That is, there exists a learning function π such that $j \in \pi(i)$ just in case the learner could acquire G_j if exposed (under normal conditions of language acquisition) to D_i, for every i and j. Given reasonable mechanistic assumptions, this function can be presumed to be effectively computable.

The construal of the learner as an effectively computable function presumes that there is a *principled* coding of data sequences and grammars by the natural numbers. Linguistic theory in conjunction with empirical psycholinguistics can presumably provide principled formal characterizations of both primary data and acquired linguistic knowledge which can then be coded using standard Gödel numbering techniques. The particular form that these characterizations may take is left open; in particular, the characterization of linguistic knowledge need not take the form of the grammars postulated by current linguistic theory.

The innateness issue separating rationalists and empiricists can be reformulated in terms of the learning function π as follows: What is the range of the function? Rationalists claim that only a severely restricted class of grammars can be acquired on the basis of primary data, which is to say that there exist certain logically possible grammars G_j such that there is no D_i such that $j \in \pi(i)$; they explain this restriction in terms of constraints on the learning function that human learners can compute. Empiricists, on the other hand, deny the existence of such restrictions on the class of available grammars, thereby committing themselves to the claim that, for every logically possible grammar G_j, there exists a D_i such that $j \in \pi(i)$. Empiricists would probably not wish to formulate their position in this way. Given their typically instrumentalist construal of grammars, they would prefer to characterize their position as denying the existence of any restrictions, other than those imposed by the learner's peripheral physiology, on the class of *languages* that can be acquired on the basis of primary data. Their characterization of their position is in fact stronger than the one I have attributed to them inasmuch as constraints on the class of available grammars need not entail (but are entailed by) restrictions on the class of languages that can be acquired.[7] Yet even on my characterization, theirs is an extremely strong position. For if, as empiricists assume, the only restrictions on the class of languages that can in

[7] See Thomas Wasow, "On Constraining the Class of Transformational Languages," *Synthese*, XXXIX, 1 (September 1978): 81–104.

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principle be acquired are those imposed by the learner's peripheral physiology and if, as they further assume, peripheral processing mechanisms do nothing more than segment the incoming acoustical signal into strings over the vocabulary of the acoustic analysis given by these mechanisms, then empiricists are committed to the claim that the full class of recursively enumerable languages defined over that vocabulary can in principle be acquired by a learner exposed to the appropriate primary data. In other words, they are committed to the claim that, for every recursively enumerable language over that vocabulary, there is a grammar G_j that generates that language and a data set D_i such that $j \in \pi(i)$. This is a claim that rationalists would wish to deny.

Provision of a finite, completely explicit specification of the learning function would decide the innateness issue separating rationalists and empiricists. Unfortunately, despite some significant progress within linguistics in characterizing the class of grammars available to the human learner, we are not likely to have such a specification in the near future. There is, however, another way of resolving the innateness issue. In recent years there has been considerable progress in formal learnability theory.[8] On the basis of this work we know something about the learnability in finite time of a certain broad class of grammars on the basis of certain types of data. These learnability results can be interpreted as providing information about the class of grammars, and hence about the class of languages, that human learners could acquire if they possessed certain sorts of "innate" knowledge or information about the class from which the grammar producing their primary data must be drawn. So interpreted, these results show both the untenability of the empiricist claim that the class of possible natural languages includes all recursively enumerable languages as well as the strong plausibility of the rationalist claim that there must be severe innate restrictions (in the form of restrictions on the class of available grammars) on the class of possible natural languages. The argument against the empiricist claim involves showing that, for their proposed characterization of primary data and the class of available grammars, there is *provably* no function that maps sets of primary data onto the grammars. The argument provided is extremely conservative inasmuch as no assumptions are made about the class of

[8] For a survey of elementary results, see A. Biermann and J. Feldman, "A Survey of Results on Grammatical Inference and Complexity," in S. Watanabe, ed., *Frontiers of Pattern Recognition* (New York: Academic Press, 1972); also, J. Feldman, "Some Decidability Results on Grammatical Inference and Complexity," *Information and Control*, xx (1972): 244–262.

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available grammars other than that it contains at least one grammar for every recursively enumerable language. Specifically, no assumptions are made about the form of the rules that the available grammars will incorporate. The plausibility argument for the rationalist claim, by contrast, is quasi-empirical in that it appeals to very general assumptions both about the nature of primary data and about the form that the grammars made available by an explanatorily adequate linguistic theory might be expected to take. This argument is nevertheless also conservative inasmuch as the assumptions in question seem empirically well supported.

An elementary theorem due to E. Mark Gold is especially relevant to our evaluation of the empiricist claim. In his seminal paper "Language Identification in the Limit,"[9] Gold investigated the learnability of different classes of formal languages within the Chomsky hierarchy on the basis of different data formats. Learning was modeled as follows: time is divided into discrete trials. At each trial t, a new datum is added to the learner's sample, and the learner then conjectures a grammar for the language from which the resulting sample S_t is drawn. A class of languages is considered learnable (in the limit) with respect to a given data format if there is an algorithm that the learner could use to make a guess at every trial t as to the particular language of the class from which the sample S_t is drawn, where the algorithm has the following property: given any language of the class, there is some finite time after which the guesses will all be the same and will furthermore be correct. Put another way, the algorithm must after some finite time always choose a grammar (not necessarily the one used to generate the sample) that generates the language from which the sample is drawn.

The theorem in question, an unsolvability theorem for so-called "text presentation," states that the class of languages that includes all languages of finite cardinality and at least one infinite language is *not* learnable in the limit on the basis of text-presentation data. Under text presentation, the sample S_t at any given time t consists of an arbitrary sequence of t strings of the language to be acquired, where the strings of the language may be construed in a variety of ways, e.g., as grammatical sentences, as sentences paired with their associated structural descriptions, as pairs of phonetic and semantic representations, etc. As $t \to \infty$, the sample comes to include all (and only) the strings of the language to be acquired. The proof of Gold's unsolvability theorem relies on the fact that

[9] *Information and Control*, x (1967): 447–474.

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the class of languages contains an infinite subset of languages $L = \{L_0, L_1, L_2, \ldots\}$ such that $L_0 = \bigcup_{i=1}^{\infty} L_i$, and, for every finite subset F of L_0, there are an infinite number of $L_i \leq L$ such that $F \leq L_i$. There is, as a consequence, no algorithm, and indeed no noncomputable function either, available to the learner which is certain to converge on any target language drawn from the class of languages in question. The reason is this: because the learner receives no information about the complement of the target language, he has no way of discovering that a hypothesized language properly includes the target language from which the data sample is drawn. He must, therefore, adopt a strategy that precludes such errors; in particular, he must at every trial t hypothesize that S_t is the target language. Such a strategy would assure convergence in the limit if the class of languages was finite. But since it is not, convergence is not assured. If the sample S_t is drawn from the infinite language L_0, then the learner will repeatedly hypothesize a language that is properly included in L_0, never correctly guessing that the target language is L_0.

The significance of Gold's result becomes apparent if one considers that (i) empiricists assume that there are no constraints on the class of possible natural languages (besides perhaps that natural languages be recursively enumerable), and (ii) Gold's result assumes that the learner employs a maximally powerful learning strategy—there are no strategies that could accomplish what that assumed by Gold cannot. These two facts, given Gold's unsolvability result for text data, effectively dispose of the empiricist claim that there exists a "discovery procedure" capable of discovering a grammar for any natural language solely by analyzing a text of that language.[10] This claim can be salvaged, but only at the price of abandoning the empiricist program, since one must abandon the assumption that the class of possible natural languages is relatively unconstrained. Some constraints are clearly in order, since by all accounts natural languages are of infinite cardinality. But the assumption that natural languages are infinitary will not suffice to ensure learnability on the basis of text data, since the class of infinite, recursively enumerable languages contains an infinite subset of languages L of the sort described above. Additional constraints

[10] See, for example, Sidney M. Lamb, "On the Mechanization of Syntactic Analysis," in *1961 Conference on Machine Translation of Languages and Applied Language Analysis, National Physical Laboratory Symposium No. 13, vol. II* (London: H. M. Stationery Office, 1962), 674–686; and Martin Braine, "On Two Types of Models of the Internalization of Grammars," in D. Slobin, ed., *The Ontogenesis of Grammar* (New York: Academic Press, 1971), pp. 153–188.

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are necessary. If the natural languages investigated thus far by linguists are representative, then the grammars generating natural languages must be at least as powerful (in weak generative capacity) as context-free, if not context-sensitive, phrase-structure grammars; however, they probably need be no more powerful than context-sensitive phrase-structure grammars.

Yet the further assumption that natural languages are context-sensitive, or even context-free, will not suffice to assure learnability on text data, since the class of infinite context-free languages (hence the class of infinite context-sensitive languages) includes an infinite subset of languages L of the sort described. Context-sensitive (or context-free) languages of infinite cardinality could, however, be acquired on the basis of a text sample drawn from the language to be acquired if there were further constraints, *known to the learner,* on either the class of context-sensitive (or context-free) languages or the ordering of the data composing the text sample. Constraints of the former sort would have to preclude the existence within the class of possible natural languages of infinite subsets of the sort that Gold's result relies on. Constraints of the latter sort would have to compensate the learner for the lack of data about the complement of the language from which the sample is drawn. If the learner "knew" that the data were ordered in a particular way, then the failure of a particular string to appear before a certain point in the sample sequence would indicate that it did not belong to the language. Precise formulations of these contraints are not relevant here; rather what is important is that a learner would be able to acquire a language of infinite cardinality on the basis of a text sample only if he had (and could use) information about these constraints. In the case of human learners such information would presumably be innate.

The fatal difficulty with empiricist accounts of language acquisition should be clear: they claim that the learner is very powerful as regards the class of languages that he can acquire, but they attribute to this learner radically insufficient internal structure (in the form of constraints on hypotheses) for acquiring that class of languages on the basis of the postulated primary data. Empiricists have failed to understand that hypotheses about input, output, and internal structure go hand in hand: an acceptable theory of language acquisition cannot attribute to the learner rich output, impoverished input, and minimal internal structure. In matters of learning, as elsewhere, one does not get something from nothing.

Recent psycholinguistic theorizing has attempted to avoid this difficulty witth the empiricist account by enriching the characteri-

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zation of primary data through inclusion of semantic information about the grammatical sentences presented in the sample. Rationalists, it is claimed, are driven to the innateness hypothesis only because they conceive of primary data as syntactic in character. If syntactic data are supplemented by semantic data, then there will supposedly be no need for significant innate structure.[11] To be sure, rationalists have typically construed primary data as syntactic in character. Chomsky, for example, concedes that semantic information may facilitate syntax acquisition; however, he doubts that such information plays any role in determining how learning proceeds.[12] Chomsky's reluctance to include semantic information, despite a number of studies that seem to indicate the relevance of such information, presumably stems from worries as to how the learner could possibly glean a sentence's meaning from the context of utterance. But these worries notwithstanding (and they should be *big* worries for someone who proposes to include semantic information in the primary data), it is useful to see just what effect the inclusion of semantic information has on learnability. This is especially important inasmuch as syntactic primary data seem appropriately modeled by text presentation: information regarding ungrammatical sentences, usually in the form of corrections of the child's syntactic errors, seemingly plays a minor role in first-language acquisition.[13]

One way of introducing semantic information would be to hypothesize that primary data are modeled by a set of ordered pairs $\langle m_i, x_i \rangle$, where m_i is the meaning (represented in some suitably formal fashion) of the data sentence x_i. Kenneth Wexler and Peter Culicover adopt this hypothesis in their learnability theory for *Aspects*-era transformational grammars.[14] That work provides what I shall argue is a conservative measure of the innate structures that must be attributed to the learner by an explanatorily adequate theory of language acquisition. The task that Wexler and Culicover set for the learner is to discover a transformational component, i.e., a set of transformations, which correctly maps deep-structure representations of the meaning of sentences onto those sentences. (Wexler and

[11] See, for example, H. Sinclair-de-Zwart, "Developmental Psycholinguistics," in D. Elkind and J. Flavell, eds., *Studies in Cognitive Development: Essays in Honor of Jean Piaget* (New York: Oxford, 1969), and I. Schlesinger, "Production of Utterances and Language Acquisition," in D. Slobin, ed., *The Ontogenesis of Grammar* (New York: Academic Press, 1971).

[12] *Aspects of the Theory of Syntax* (Cambridge, Mass.: MIT Press, 1965), pp. 33–34.

[13] See R. Brown and C. Hanlon, "Derivational Complexity and the Order of Acquisition of Child Speech," in J. R. Hayes, ed., *Cognition and the Development of Language* (New York: Wiley, 1970).

[14] *Formal Principles of Language Acquisition* (Cambridge, Mass.: MIT Press, 1980).

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Culicover assume that the learner is able both to generate the base phrase-markers that serve as the deep-structure representations of meanings and to map particular meanings onto their appropriate base phrase-marker representations.) If the learner is to succeed in this task, then the class of possible transformational components must be constrained so as to ensure that the learning procedure employed by the learner will converge on the correct transformational component. The nature and severity of the constraints will depend on the sort of learning procedure imputed to the learner. If, for example, the learner is assumed to employ an enumeration algorithm, then the class of possible transformational components must be effectively enumerable. One way to ensure this is to constrain the applicability of transformations in such a way that there will be only a finite number of phrase-markers eligible for transformation. The particular constraints proposed by Wexler and Culicover have this effect, even though their theory imputes to the learner a different, nonenumerative procedure. Wexler and Culicover reject enumeration algorithms as psychologically implausible on a number of grounds, notably because such algorithms assume both that the learner has an unbounded memory for past data and that he rejects incorrect hypotheses wholesale rather than modifying them piecemeal. Their learning procedure, which incorporates neither of these assumptions, works as follows: given a datum pair $\langle b, s \rangle$ consisting of a base phrase-marker b and its derived surface-structure string s, the procedure applies the currently hypothesized transformational component C_i to the base phrase-marker b. If it succeeds in deriving the surface string s from b, then it maintains its current hypothesis and awaits a new datum pair. If it fails to derive s from b, deriving instead some $s^* \neq s$, then either the learning procedure rejects one of the rules in C_i used to derive s^* or it adds to C_i a rule that, when applied at the topmost cycle in the derivation, yields a phrase-marker that has s as its terminal string. The particular modifications to C_i effected by the learning procedure are nondirected; consequently, incorrect rules will sometimes be added to C_i, and correct rules will sometimes be rejected from C_i. The constraints imposed on the class of possible transformational components must ensure that the probability of these modifications eventually converging on the correct transformational component is bounded away from zero. The crucial requirement for ensuring a nonvanishing probability of convergence is that the learner not have to wait for a datum pair containing a base phrase-marker of arbitrary complexity (where complexity is measured by the degree of sentence embedding) in order to discover that a hy-

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pothesized transformational component fails to provide a correct mapping of base phrase-markers onto surface strings. More precisely, the constraints must ensure that, if there exists a detectable error in the mapping of an arbitrarily complex base phrase-marker onto a surface string, then an error is detectable in the mapping of a base phrase-marker of bounded complexity onto a surface string.

Wexler and Culicover introduce a number of constraints on the applicability of transformations in order to preclude the possibility that certain errors in the mapping effected by a hypothesized transformational component will be detectable only on complex data. One fundamental constraint, the so-called "binary principle," permits a transformation to analyze structures at the level of sentence embedding at which it is operating, plus the next lower level, but no further down. By preventing transformational rules from analyzing structures of unbounded complexity, one precludes the possibility that certain transformations would be exhibited only in derivations involving complex phrase-markers. Other constraints are introduced in order to preclude various interaction effects that would similarly be exhibited only in derivations involving complex phrase-markers. The "raising principle," for example, requires that, if a node is raised transformationally from one level to another, then structure under that node may not be analyzed (and thus changed) by subsequent transformations. This constraint precludes the possibility that an error produced by an earlier (incorrect) transformation and hidden in the raised structure would be detectable only after this structure had been raised into a context that permits the application of a transformation that reveals the error. Without this constraint the error-containing structure might have to be raised through an arbitrary number of levels before the error was detectable. A related constraint, the "freezing principle," requires that, if a transformation changes the structure under a node so that it is no longer base-generable, then that structure may not be analyzed (and hence changed) by subsequent transformations. This constraint limits the extent to which base phrase-markers can be deformed by derivation, thereby limiting the extent to which one can by means of a lengthy derivation develop the structure necessary for the application of an error-producing or error-revealing transformation. Other constraints introduced to eliminate interaction effects function similarly to preclude the possibility that the applicability of error-producing or error-revealing transformations could be contingent on an arbitrarily complex derivation that develops the structures to which those transformations apply. The cumulative effect of the various constraints introduced by Wexler

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and Culicover is to ensure that the transformational mapping of base phrase-markers onto surface strings is insensitive to the complexity of the phrase-markers over which the mapping is defined. Nothing happens in derivations involving complex phrase-markers that does not also happen in derivations involving phrase-markers of low complexity. In fact, Wexler and Culicover are able to prove that a learner employing their learning procedure can acquire any member of the class of transformational components satisfying those constraints on the basis of primary data of "degree-2" complexity (i.e., the base phrase-markers in the datum pairs involve no more than two levels of sentence embedding). The procedure has "innate" knowledge of these constraints inasmuch as it is designed to hypothesize only components that satisfy these constraints.

The constraints proposed by Wexler and Culicover are certainly nontrivial. It is a relatively simple matter to prove that these constraints make available to the learner only a *finite* number of transformational components. Without constraints the theory of transformational grammar presented in *Aspects* makes available infinitely many transformational components. So powerful is the theory, Stanley Peters and R. W. Ritchie have proved that every recursively enumerable language is weakly generated by some context-free based transformational grammar of the sort characterized in *Aspects*; indeed, these languages can be generated even when the theory satisfies the Universal Base Hypothesis, which implies that the power of the theory resides in the relatively unconstrained mappings that it permits of base phrase-markers onto surface strings.[15]

Learnability of a class of transformational grammars can obviously be achieved in ways that do not entail the specific constraints proposed by Wexler and Culicover, since these constraints reflect (1) assumptions about primary data, (2) assumptions about the learning procedure, (3) the particular theory of transformational grammar that they adopt, and (4) assumptions that are introduced in order to facilitate their learnability proof. Nevertheless these constraints provide a conservative measure of the innate structures that must be attributed to the learner by any explanatorily adequate theory of language acquisition. Assumptions about both primary data and the learning procedure seem safely conservative in that psychologically more plausible assumptions will surely necessitate further constraints on the class of grammars made

[15] "On the Generative Power of Transformational Grammars," *Information Science*, VI (1973): 49–83; and "A Note on the Universal Base Hypothesis," *Journal of Linguistics*, V (1969): 150–152.

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available to the learner. Although the learner may be able to glean the meanings of simple sentences (or at least parts of sentences) from context, it seems implausible to suppose that he can do this for all degree-2 sentences. Yet to the extent that one impoverishes the primary data available to the learner by restricting his ability to recover the meaning of certain sentences he hears, one must compensate the learning procedure by providing more information about the class of possible transformational components. One should anticipate similar effects as the learning procedure is made psychologically more plausible. Just as Wexler and Culicover's learning procedure requires more constraints on the class of transformational components than does a simple enumeration algorithm, a procedure in which modifications to hypotheses are directed rather than largely random will surely require further constraints on the class of possible transformational components, inasmuch as directed modifications presume that responsibility for specific errors (or sorts of errors) can be assigned to specific rules (or sorts of rules). One would not expect any arbitrary class of transformational components to exhibit this property; rather one would expect that such a property would result from fairly specific constraints on the class of components available to the learner. The various cognitive, developmental, and real-time assumptions that a psychologically plausible learning procedure would have to satisfy can be expected to levy further constraints on the class of possible transformational components, since such assumptions effectively limit the power of the procedure.[16] Such limitations must be compensated for, either by enriching the input or constraining the output.

The constraints proposed by Wexler and Culicover clearly reflect the particular theory of transformational grammar that they adopt, namely, a version of so-called "Standard Theory"; nevertheless, one might reasonably expect that any explanatorily adequate alternative would exhibit constraints similar in both severity and consequence to those proposed. This is because any such theory must make available for every natural language a descriptively adequate grammar that can be acquired on the basis of appropriate primary data. But however a theory conceives of the mapping between sound and meaning or between surface strings and their meanings, it will have to account for roughly the same linguistic phenomena that motivated Standard Theory; moreover, it will have to do so in

[16] See Wexler and Culicover, *op. cit.*, chs. 1–2; also Steven Pinker, "Formal Models of Language Learning," *Cognition*, VII (1979): 217–283.

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a way that satisfies Wexler and Culicover's requirement that errors detectable in the mapping of an arbitrarily complex representation of meaning onto a surface string be detectable in the mapping of some representation of bounded complexity onto a surface string. These requirements are fairly restrictive; given the formal resources available for characterizing the recursive structure of natural languages, theories satisfying these requirements are going to be similar with respect to both the severity and the consequences of the constraints that they impose on the class of grammars made available to the learner. It is noteworthy that both the revised "Extended Standard Theory" presently defended by Chomsky and the lexical-interpretive theory of Joan Bresnan share with the constrained version of Standard Theory proposed by Wexler and Culicover the feature of making available to the learner only a finite number of possible grammars.[17] These alternative theories also contain locality constraints on movement, binding, and the like which are similar in effect to Wexler and Culicover's binary principle. There are, of course, important differences between the constraints introduced by these different theories. Wexler and Culicover's constraints, for example, are on the applicability of transformations, whereas more recent theories tend to constrain the form of transformations. Yet, from the point of view of learnability considerations, these different constraints come to much the same thing, since both effectively reduce possible interaction effects in transformational derivations by limiting the context-dependency of transformations. If, for example, one has a transformational component consisting only of rules of the form "Move α," where α is a variable ranging over certain grammatical categories (e.g., NP, WH, etc.), then one does not have to worry about interaction effects of the sort that concerned Wexler and Culicover, since the application of rules of this form is not context-sensitive.

The burden of the foregoing considerations has been to show the sense in which the language that each of us acquires is, in Chomsky's words, "a rich and complex construction hopelessly underdetermined by the fragmentary evidence available." The fact that natural languages are nevertheless acquired can seemingly be explained only on the assumptions that there are severe limitations on the class of possible natural languages in the form of constraints on the class of available grammars and furthermore that the learner comes to the

[17] See Chomsky, *Lectures on Government and Binding* (Dordrecht: Foris, 1981), and S. Pinker, "A Theory of the Acquisition of Lexical-interpretive Grammar," in J. Bresnan, ed., *The Mental Representation of Grammatical Relations* (Cambridge, Mass.: MIT Press, 1981).

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learning task equipped with information about these constraints. The argument that I have sketched in support of this conclusion assumes that modern linguistic theory is largely correct in its assumptions regarding the conditions that a descriptively adequate grammar for a language must satisfy. The argument, however, is not committed to a transformational framework. Recent proposals that utilize nontransformational machinery to accomplish the descriptive tasks that motivated the introduction of transformations in existing theory present similar learning-theoretic difficulties for the learner: the generative power of this machinery must be severely constrained and the learner informed of these constraints if he is to be able to acquire any of a broad class of natural languages on the basis of access to available primary data. One gets some measure of the generality of the assumptions on which the foregoing argument is based if one recalls that the argument against the empiricist account involved showing that there exists no device capable of acquiring the full class of recursively enumerable languages on the basis of text data. That result does not depend on any assumptions about the grammatical characterization of these languages (which explains, incidentally, the usual construal of learnability results as having to do with languages rather than grammars, even though these results construe learning as the acquisition of a grammar). It is the generality of the assumptions on which my argument is based that distinguishes it from a version of the poverty-of-the-stimulus argument employed in recent linguistic argumentation.[18] On that version, proposed principles of universal grammar are defended by showing that one can explain (deductively) the acquisition of a particular grammatical rule or feature on the basis of primary data if one assumes that the learner knows innately the hypothesized principle of universal grammar. Such arguments do not establish the necessity of the hypothesized principle inasmuch as it would suffice for the acquisition of the rule or feature in question that the learner know certain other principles that entailed the first; nevertheless, these arguments may be helpful in constructing a theory of universal grammar. But whatever their heuristic merits, such arguments hardly provide uncontentious support for the presumed conclusion to the poverty-of-the-stimulus argument, since they presuppose that the grammatical rule or feature in question is part of a correct characterization of the learner's eventual grammatical competence. Those unwilling to accept the presupposition

[18] See, for example, N. Hornstein and D. Lightfoot, eds., *Explanation in Linguistics* (London: Longmans, 1981).

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could hardly be expected to accept these arguments as anything more than a characterization of the innateness commitment entailed by the presupposition.

The considerations adduced in support of the poverty-of-the-stimulus argument may seem to fall short of establishing the plausibility of the innateness hypothesis, since it is compatible with the conclusion of the argument that the relevant information about constraints on the class of possible grammars is not innate. Perhaps the learner acquires this information through prior learning. In fact, however, this seems highly implausible for just the reasons that lend strong support to the poverty-of-the-stimulus argument. It seems unlikely that the learner could induce these constraints from the data to which he has access before language acquisition. Linguistic data would not be helpful, since even if sufficient data were available, the learner acting as a "little linguist" could induce the relevant constraints only if he already understood the language or languages from which these data were drawn. Nonlinguistic data would be no more helpful, since recent research in psychology and artificial intelligence strongly suggests that the constraints on learning and recognition procedures are highly domain-specific.[19] The learner is not going to be able to induce the constraints in question from nonlinguistic data for the simple reason that such data do not exhibit them. It would seem, therefore, that information about constraints on the class of possible grammars must be innate rather than acquired. To say that these constraints are innate is not to say that the structures instantiating these constraints must be present at birth. Rather it is to say that the existence of these structures at the time that language is acquired is not to be explained as being the result of "learning from experience" in any known sense of this expression. These structures may well be the result of a maturational process of some sort, and their emergence may well be triggered by certain stimuli.

Rationalists and their critics have long recognized that linguistic universals are relevant to the innateness hypothesis, but they have not always understood precisely how they are relevant. The foregoing development of the poverty-of-the-stimulus argument sheds some light on this matter. Rationalists, who insist upon the evidential support provided by the discovery of linguistic universals, are often taken to be offering an argument from the existence of such universals which has the following form: there is no plausible

[19] See, for example, Shimon Ullman, *The Interpretation of Visual Motion* (Cambridge, Mass.: MIT Press, 1979).

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alternative to the innateness hypothesis which can explain the existence of linguistic universals; therefore, the innateness hypothesis is at least plausible. Rebuttals of this presumed argument attempt to show that whatever universals do exist can be explained in ways that do not entail the innateness hypothesis.[20] Although versions of this argument can be found in the writings of some rationalists, the appeal to linguistic universals is, I believe, better understood as an appeal to empirical evidence for the innateness hypothesis. That hypothesis predicts the existence of linguistic universals that reflect the innate constraints on the class of available grammars that enable the learner to acquire a language on the basis of available primary data,[21] and rationalists take the discovery of certain linguistic universals to bear out this prediction. Two evidentiary conditions deserve mention. First, only linguistic universals the existence of which is predicted on learning-theoretic grounds will support the hypothesis. Second, the discovery of such universals provides support for the hypothesis only if these universals are not otherwise to be expected. Although the universals that rationalists typically cite (the structure-dependency principle, the subjacency condition, etc.) are certainly unexpected and nonintuitive, they are not obviously predicted on learning-theoretic grounds. Wexler and Culicover are noteworthy among rationalists for their attempt to demonstrate that the universals that they cite in support of the innateness hypothesis (e.g., the binary and freezing principles) are in fact entailed by their acquisition theory. Most rationalists leave this point unargued because they assume that all universals are learning-theoretically significant. But this is simply mistaken. A particular universal will be significant only if the acquisition procedure employed by the learner would fail to acquire at least one natural language on the basis of appropriate primary data if the class of available grammars failed to satisfy the universal in question. It is perhaps a sound research methodology to assume that all linguistic universals are learning-theoretically significant; however, this as-

[20] See, for example, Putnam, *op. cit.*, pp. 15–19; and David E. Cooper, *Knowledge of Language* (Atlantic Highlands, N.J.: Humanities, 1975), pp. 160–182.

[21] In his paper "Empiricism, Innateness, and Linguistic Universals," *Philosophical Studies*, XXXIII, 3 (April 1978): 273–286, Stephen Stich points out that the existence of substantive universals is not entailed by the innateness hypothesis alone, since specific innate constraints on the class of possible natural languages might be shared only by specific subclasses of possible natural languages. The prediction of linguistic universals therefore depends on additional premises, presumably about the biological basis of human linguistic capacity. In any event, the discovery of linguistic universals satisfying the two evidentiary conditions described below would provide evidence for both the innateness hypothesis and the required biological assumptions.

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sumption cannot warrant construing every linguistic universal as empirical evidence for the innateness hypothesis.

Empiricists, for their part, often claim with little argument that the universals discovered by rationalists can be explained away on other grounds and that the innateness hypothesis is therefore without support. Even if they are right that these universals should be expected on other grounds, this fact would leave the poverty-of-the-stimulus argument untouched. And it is this argument, and not presently available empirical evidence for the existence of learning-theoretically significant universals, that at present provides compelling support for the innateness hypothesis.

III. THE RATIONALIST COMMITMENT TO THE INTENTIONAL IDIOM

The poverty-of-the-stimulus argument would appear to provide strong support for the innateness hypothesis: there are severe constraints on the class of languages that can be acquired on the basis of given access to primary data, and these constraints are levied by innate principles that determine the grammatical form of the languages that can be acquired. What remains to be considered is the further rationalist claim that what is innate is not simply certain mechanisms of learning, but certain knowledge or ideas (what I am calling "innate content"). Empiricists find this claim puzzling, especially when it is coupled with a commitment to token physicalism, for it seems to entail a dualism incompatible with physicalism.

In fact the claim is epistemological rather than ontological, though not epistemological in the sense of being of a sort that a philosophical theory of knowledge might resolve. Little hangs on the rationalist's use of the word 'knowledge' as a characterization of innate content. The issue has rather to do with assumptions about the nature of any explanatorily adequate theory of learning, specifically about the theoretical vocabulary to be employed. Rationalists are seemingly committed to propositional-attitude theories of learning. The hypothesis-testing idiom in terms of which rationalist accounts of language acquisition are often couched manifests that commitment. On these accounts, the learner is characterized as knowing certain things, as selecting certain hypotheses from the set of possible hypotheses, as testing those hypotheses against primary linguistic data, as choosing the optimal grammar from among those compatible with the data, as using an evaluation measure, and so on.[22] Rationalists do not deny that there are any number of other, nonintentional descriptions of the development of linguistic ability in the child, but they believe (hypothe-

[22] See, for example, Chomsky, *Aspects*, pp. 81–104.

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size) that the appropriate vocabulary for an explanatorily adequate theory of language acquisition will characterize the relevant internal states and processes of the learner in terms of his knowledge, beliefs, assumptions, and the like. The intentional idiom of propositional attitudes is thought to characterize the etiology of behavior at a level of abstraction appropriate for capturing the behaviorally relevant similarities common to the possibly disparate physiological processes that eventuate in a single type of behavior; this idiom is thought to provide the appropriate theoretical vocabulary for capturing significant nomological generalizations about the etiology of behavior.[23]

The rationalist claim for the existence of innate content follows directly from the commitment to the intentional idiom: inasmuch as one can explain how a learner is able to acquire a language on the basis of primary data only by assuming that he possesses rich innate internal structure, an adequate theory of language acquisition will have to characterize this innate structure. But since the rationalist account will be couched in the idiom of propositional attitudes, the characterization will be in these terms. Now, within this idiom the behaviorally relevant internal states of an organism are individuated in terms of both the *content* of the state and the organism's *relation* to that content, and so a characterization of such states will mention both a content and a relation (e.g., knowing that p, desiring that q, etc.). Hence, a rationalist characterization of the innate structures that enable language acquisition will take the form of a specification of a content and a relation to that content. Thus, for example, if on the basis of linguistic research we hypothesize that all natural languages satisfy the so-called "subjacency condition," and if we further hypothesize that this fact about natural languages reflects a constraint imposed by the innate structure of human linguistic capacity, then a rationalist account will characterize the learner as standing innately in a certain relation to the content "that all natural languages satisfy the subjacency condition." When, as is often the case, rationalists characterize the learning as *knowing* innately that, for example, all natural languages satisfy the subjacency condition, this is a further conjecture on their part as to the appropriate characterization of the relation that the learner bears innately to that content. Such a conjecture, of course, is open to whatever epistemological criticisms can be

[23] For a general defense of this view, see Jerry Fodor, *Representations* (Cambridge, Mass.: MIT/Bradford, 1982) and Zenon Pylyshyn, *Cognition and Computation: Toward a Foundation of Cognitive Science* (Cambridge, Mass.: MIT/Bradford, 1984).

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brought against claims to innate knowledge; however, the crucial point here is this: the rationalist's insistence that content as well as mechanism are innate expresses a commitment to the intentional idiom of propositional attitudes as providing the appropriate characterization of at least some innate internal structure. Intentionally characterized states of the learner, including the innate content postulated by rationalist accounts of language acquisition, are not something that exists in addition to innate machinery or mechanism, what for lack of a better word I have been calling "internal structure." Rather, intentionally characterized states are, as the terminology would suggest, intentional characterizations of that structure.

It is, I think, premature to attempt to assess the plausibility of the rationalist commitment to a propositional-attitude account of language acquisition. Whether this idiom provides the appropriate theoretical vocabulary for an explanatorily adequate theory of language acquisition is a strictly empirical question. Rationalists take it to be some measure of the appropriateness of the intentional idiom that the explanations and predictions of folk psychology are couched in it: they argue that the predictive successes of folk psychology can be explained only by assuming that it provides an approximately correct account of the etiology of the predicted behaviors. The idiom of propositional attitudes is claimed to provide an approximately correct taxonomy of the kinds of states, processes, and events that figure in this etiology, a taxonomy that permits a characterization of the etiology of behavior at a level of abstraction appropriate for capturing the significant nomological generalizations. Whatever plausibility such considerations may lend to propositional-attitude psychology in general, they would seem to provide little support for such accounts of language acquisition. Folk psychology is notably silent on matters of learning: it tells us that learning takes place under appropriate conditions, but it offers only the vaguest characterization of these conditions. It does provide a variety of ready explanations of learning failures; however, these explanations do not seem any more committed to the idiom of propositional attitudes than are explanations of failures to master certain skills. The problem here is not simply that there is no general folk theory of learning to which folk explanations might advert; folk psychology might well have provided different theories for different domains of knowledge. Rather the point is that, for all folk psychology tells us, learning might be akin to muscle-building, which is to say that an explanatorily adequate account of language acquisition would employ a nonintentional, perhaps neuro-

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physiological vocabulary, describing learning as the development of certain brain functions under conditions of sensory stimulation. Such an eventuality is entirely compatible with a propositional-attitude account of linguistic behavior: nothing precludes the possibility that nonintentional processes should eventuate in an intentional system. To suppose that it must is to commit a species of genetic fallacy.

The development of a nonintentional account of language acquisition would leave us with something differing from empiricism in its commitment to a rich innateness hypothesis, yet differing from rationalism in its abandonment of the intentional idiom. Some will see this third alternative as but a more resourceful empiricism. Empiricists, they will argue, can with clear conscience accept an innateness hypothesis that imputes to the learner an innate structure substantially richer and more constraining than that envisioned by associationist theories. But such a position would be empiricist only in name. Abandoning associationist theory, which has historically been the centerpiece of empiricist psychology, would presumably require abandoning as well the notion that there can be a general theory of learning. A resourceful empiricist would presumably hypothesize that linguistic capacity is task-specific, since what is learned in acquiring a language seems sufficiently different from other acquired knowledge to make the innate structure necessary for language acquisition of little use in other domains of learning. He would presumably also hypothesize that linguistic capacity is species-specific, since any hypotheses he might entertain regarding innate structure would depend heavily on assumptions about the nature of primary data, which clearly do not provide an adequate basis for language acquisition in nonhumans, at least not under similar conditions of learning. In none of this is there the demeanor of a well-bred empiricist.

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COMMENTS AND CRITICISM

ON DEVITT ON DUMMETT*

RECENTLY, Michael Devitt[1] has claimed that Michael Dummett's argument for anti-realism relies on unsupported assumptions, false premises, and bad scientific theories. Consequently, he rejects the argument as "bad from beginning to end" (97). In this paper, I will not attempt a reconstruction, let alone an evaluation, of Dummett's position (though, of course, some of my remarks will bear on these), nor will I discuss Devitt's own brief proposals. Rather, I will be concerned with Devitt's critique, which I find quite unsatisfactory (for, at times, illuminating reasons).

I

In the first section of his paper, Devitt attempts to argue that Dummett holds what he calls premise A: "The Realism dispute is the dispute about whether statements have realist (evidence-transcendent) or only verificationist truth conditions" (74), that premise A is false, and that Dummett's arguments for it are bad. Devitt's argument consists in describing a doctrine he calls "Realism": "Common-sense physical entities objectively exist independently of the mental" (76), a doctrine of "Realist Truth": "Physical statements are true or false in virtue of: (i) their objective structure; (ii) the objective referential relations between their parts and reality; and (iii) the objective nature of that reality" (77), and arguing that (a) Realism does not entail Realist Truth and (b) Realism does not follow from Realist Truth.

To begin with, it is unclear why Devitt argues for (a) since he is aware (81) that Dummett has often argued that the road from a position on ontology to one on semantics is closed, or at least cannot be travelled without great cost (see, for example, EI §7.1 and TOE

* Many thanks to Stephen Blacklocks and James Higginbotham, whose kind and insightful comments on an earlier version made this paper more enjoyable to write and read. The final draft was written while I was a Fulbright Scholar at New College, Oxford; my appreciation to the American and British peoples for that opportunity.

[1] "Dummett's Anti-Realism," this JOURNAL, LXXX, 2 (February 1983): 73–99; page references in parentheses are to this article. Page references following "TOE" are to Dummett's *Truth and Other Enigmas* (Cambridge, Mass.: Harvard, 1978); those following "EI" to his *Elements of Intuitionism* (New York: Oxford, 1977); those following "WTMI" to his "What Is a Theory of Meaning? (I)" in Samuel Guttenplan, ed., *Mind and Language* (New York: Oxford, 1975): 97–138; and those following "WTMII" to his "What Is a Theory of Meaning? (II)" in Gareth Evans and John McDowell, eds., *Truth and Meaning: Essays in Semantics* (New York: Oxford, 1976): 67–137. For further references, see Devitt's article.

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231-247). This should have shaken his confidence that Dummett simply *equates* the two issues, as premise A does. Furthermore, Devitt's attempt to show that Dummett is committed to A is not successful. He cites passages where Dummett does seem to identify the two disputes, as well as passages where he weakens his claim to something like the negation of (b). Charity, as well as the realization that Dummett supports (a), should have led Devitt to weaken A.

So let us assume that Dummett's point is that Realism "rests upon" the adoption of a truth-conditional semantics for our language. Devitt's entire argument for (b) consists in pointing out that

> An idealist who believed in the objective existence of a purely mental realm of sense data could subscribe to Realist Truth. He could believe that physical statements are true or false according as they do or do not correspond to the realm of sense data (77).

This argument is at best inconclusive and at worst unfair, depending on which of the, apparently conflicting, positions Dummett has taken over the years on the compatibility of reductionism with realism one considers. Devitt claims, it seems, that a notion of realist truth is applicable to a class of statements for which a reductive class (TOE 156) exists and then concludes that a realist semantics does not force a realistic attitude toward the objects referred to by the statements of that first class. In his early paper "Realism", Dummett assumed that the success of phenomenalism would spell the failure of realism with respect to material objects (TOE 157). He argued that this realism may still be in trouble even if phenomenalism cannot be made to work, if a verificationist semantics is seen to be most appropriate for the class of material-object statements. Since, then, the whole issue of which semantics to adopt becomes relevant to the metaphysical debate only after it is agreed that material-object statements do not form a reducible class, that is, after it is agreed that phenomenalism has failed, Devitt's argument, making use as it does of the possibility of such statements' being true in virtue of the truth of sense-data statements, is unfair.

However, in more recent writings (see, e.g., WTMII 93/4), Dummett holds that reductionism with respect to a class of statements is compatible with the adoption of a realistic attitude toward the objects mentioned in them. Once we recall that reductionism is, for Dummett, not a metaphysical or ontological position but, rather, a semantic one, we realize that the above characterization of Devitt's position needs supplementing since, as it stands, it has him assuming the incompatibility of realism with a certain semantic thesis, whereas his view is that "Theories of language and understanding should not determine theories of the world" (75). The

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supplementation required is that the idealist in his example believes "in the objective existence of a purely mental realm of sense data." This, according to Devitt, is what is incompatible with a realistic attitude toward material objects. But, now, one can clearly see that the issue really boils down to whether Devitt's formulations of realism and idealism have clear content or whether they are, as Dummett might suggest, more or less metaphorical, awaiting an interpretation that will give them some bite. By itself, then, Devitt's argument for (b) is wholly inconclusive, since it is completely dependent on his rejection of Dummett's skepticism of such questions' being contentfully discussed in these terms.

Devitt does address this issue when he turns to an evaluation of "the metaphor thesis" (79–83); I will voice only one complaint about his discussion. This is that Devitt mislocates, it seems to me, Dummett's reasons for urging us to couch the issues in semantic terms. He claims that "Dummett's view of metaphysics comes from his philosophy of mathematics (TOE xxiv)" (80) (the page Devitt cites provides no support for this claim, as far as I can tell). Actually, Dummett's views find their source in his belief that "the characteristic tenet" of "the analytical school of philosophy", of which he takes himself to be a member, "is that the philosophy of language is the foundation for all the rest of philosophy" (TOE 454); this is so "because it is only by the analysis of language that we can analyse thought" (TOE 442). Furthermore, he believes that couching the traditional disputes in semantic terms holds out the hope that philosophical debate "can be carried on in such a way, not, indeed, that disputes do not arise, but that they can be resolved to the satisfaction of everyone" (TOE 454), that "future research [may] proceed according to more or less agreed methods of enquiry, and [that] its results [. . .] be judged in accordance with generally accepted standards" (TOE 455). (In general, see the last essay of TOE, especially 441/2 and 454–458.) Since Devitt does not venture into these deep and murky waters or even show any awareness that they provide the source for Dummett's "view of metaphysics" and espousal of "the metaphor thesis," his discussion fails to meet Dummett's head on.

II

In the second section of his paper, Devitt turns to what he calls "the propositional assumption about competence" (83–90). There he distinguishes between two versions of the other premises of his reconstruction, premises B: "The dispute about truth-conditions is the dispute about whether the competent speaker's understanding is realist (evidence-transcendent) or only verificationist" (75), and

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C: "The competent speaker's understanding is only verificationist" (75), charges that Dummett conflates the two versions of premise B and the two versions of premise C, and argues that, in any event, on the first version, both premises B and C are false. The first version analyzes understanding in terms of propositional knowledge; the second construes it as some practical ability.

Devitt's charge exhibits a fundamental misunderstanding of Dummett's position. The question that continually exercises Dummett is how attributions of knowledge can enter into an explanatory account of our use of language. His concern is that such attributions may doom the account to vacuity or circularity. In the case of conscious or verbalizable knowledge, this threat can be met, since we can always test possession of such knowledge, inferred from behavior and attributed to the agent as part of an explanation of that behavior, by querying him or her. The problem begins when knowledge of meaning is at issue; for then, according to Dummett, we cannot without circularity take verbalizable knowledge as the model for understanding, and the mastery of a language could not consist in the ability to give circular explanations (EI 373). For this reason, if we adhere to the view that understanding should be construed as knowledge of conditions of correct assertibility (no matter how *these* are characterized), we are forced to consider part of our understanding of language as consisting in implicit or nonverbalizable knowledge. But since such knowledge is, by its nature, inaccessible to querying, we now run the risk of attributing this knowledge in explanation of linguistic behavior when the only evidence we could have for its possession is that the speaker performed that behavior. Without some independent characterization of unconscious knowledge, our theories will be deprived of any explanatory force; hence the request to state in what this tacit knowledge consists. Devitt, however, assumes throughout that we understand what it is to attribute implicit propositional knowledge to a speaker (and that the only problem is that this is the wrong way to analyze a speaker's "competence in" his language); for Dummett, the problem is that we really do not understand what it is to speak of such knowledge. Dummett proposes that this knowledge consists in the capacity to recognize when the conditions of correct assertibility are met; in this way he takes himself to have restored explanatory value to those theories which make use of tacit knowledge. The extent to which Devitt completely misses all this is demonstrated in the following: "Dummett thinks that part of this knowledge is only "implicit" or "tacit" [. . .]. This use of weasel words in stating the propositional as-

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sumption casts doubt on Dummett's commitment to it" (84/5). It is because Dummett believes he has offered decisive grounds for using these "weasel words" that he gives some teeth to the antecedently contentless propositional assumption by talking of certain capacities the speaker-hearer has. Dummett may be wrong; but there is not a hint of this more interesting interpretation in Devitt, who insists that Dummett has simply conflated two intelligible positions (for some relevant passages, see TOE 128/9, WTMI 112, and WTMII 80–83, 128).

In addition, this interpretation makes certain of Dummett's remarks which baffle Devitt seem rather natural. In a footnote, Devitt writes that

> Dummett also makes the mysterious remark that a theory of meaning, which is for him a theory of understanding, is not "a psychological hypothesis" partly on the ground, it seems, that something with "internal mechanisms" unlike ours, e.g., a Martian or a robot, might have the required implicit knowledge [. . .]. This overlooks the distinction between psychological and physical mechanisms. A Martian or a robot that is physically different from us might be psychologically the same. The remark is mysterious because if understanding is anything it is (at least, partly) psychological (85).

The mystery disappears once one realizes that understanding, interpreted as tacit knowledge, is being taken to consist in a capacity to behave in a certain way which a Martian or a robot might have as well, irrespective of how greatly their physical or functional organizations differed from ours. The mystery persists so long as one thinks of understanding, interpreted as tacit knowledge *tout court*, to be a notion of possible explanatory value whose clear nature determines it to be a psychological notion. Someone who believed in the explanatory adequacy of functionalist accounts in psychology would argue that Dummett's is a bad example, if we interpret 'internal' to mean "physical" (rather than "physical or functional"). However, either Dummett is not such a believer or he is, in which case we should not so interpret 'internal'. Either way, the remark fails to be mysterious once one understands how Dummett understands 'understands'.

Next Devitt turns to the question whether there are any good arguments for the propositional assumption, in particular, whether Dummett has any to offer (85–88). He proceeds by outlining a simple-minded six-step argument "suggested by Dummett's discussion of Frege's distinction between sense and reference (TOE 117–126; particularly 124–126)" (86), arguing that it is dubious from start to

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finish, and then, though not attributing this argument to Dummett, claiming that "if this, or something like it, is not his implicit argument for [the propositional assumption], then he has offered not even the glimmer of an argument for" it (87/8). Dummett's rather involved arguments for the propositional assumption can be found in the passages cited as well as in WTMII, especially sections I and VI.[2] Since it is impossible for me to summarize them here, I leave it to the reader to determine whether indeed Devitt's six-stepper is "suggested" by these passages and, failing this, whether they lack "even the glimmer of an argument."

In the last part of this section, Devitt turns to assessing the "propositional assumption" (88–90) and declares that what he is "most intent on denying is that competence requires primarily, and in general, semantic propositional knowledge, whether implicit or explicit" (88). (Note, again, the un-Dummettian assumption: there are no particular difficulties with the notion of "implicit semantic propositional knowledge" since such knowledge is just like the explicit kind, only not explicit.) I will make only two observations about this. First, as I pointed out above, from a Dummettian viewpoint, arguing against the "propositional assumption" *tout court* is a little like arguing against reincarnation. Devitt has not given any model or characterization of what tacit propositional knowledge is that would permit it to enter into explanatory accounts of language use. This is in part due to lack of attention to the assumption of the unverbalizability of this knowledge and its consequences (e.g., he characterizes the "propositional assumption" as claiming that a speaker's competence consists in "*having thoughts about*" his language (88); our thoughts most often (some would say "always") are verbalizable, if not conscious).

Second, from a Chomskyan position,[3] the position of someone who holds that attributions of unconscious propositional knowledge do play a role in explanatory theories, Devitt's argument is not in the least compelling. His objection is summarized in the following passage:

> . . . a person could not have semantic propositional knowledge without having the semantic vocabulary of some language. That vocabulary is an isolable part of a language, just as is the biological or eco-

[2] See also Dummett's "What Do I Know when I Know a Language?," lecture delivered at the Centenary Celebration of Stockholm University, May 24, 1978, issued as a pamphlet by Stockholm University.

[3] See, e.g., Noam Chomsky's *Rules and Representations* (New York: Columbia, 1980), p. 93.

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> nomic vocabulary. A person could be competent in the nonsemantic part of a language without being competent in its semantic part or in the semantic part of any other language. So competence in the nonsemantic part does not consist in semantic propositional knowledge. So competence in the language as a whole does not either (88).

Neglecting the fact that it is unclear what Devitt means by "being competent in part of a language," his argument assumes that "semantic propositional knowledge" requires "competence in" some other language. However, if, in accordance with some Chomskyans, "semantic propositional knowledge" is taken to be particular abstract structures and configurations of the human brain together with the principles that relate and transform these, then someone could possess this knowledge without having "the semantic vocabulary" of *any* language. However, this argument may be unfair: if competence is taken to be states of our brain, then, since it would seem there is nothing that we *have*, only states in which we *can be*, in what sense is competence knowledge that we possess? But Devitt's argument still fails against those Chomskyans[4] who do not believe that the subject matter of linguistic theory is brain states and processes. Devitt simply assumes that competence in a language does not require competence in the semantic vocabulary of any language. However, if competence is interpreted as real knowledge that one possesses, then it is rather mysterious how one could have it without having the relevant semantic vocabulary. Devitt's argument simply begs the question against such a view. In short, Devitt's remarks seem either empty or invalid depending on whether one views them from a Dummettian perspective or from a powerful un-Dummettian one.

III

In the third section of his paper (90–97), Devitt offers a reconstruction of an argument for the claim that a sentence understood by a competent speaker has only verificationist truth conditions. The rest of my remarks are devoted to an assessment of Devitt's rejection of this argument, rather than of the reconstruction itself, which I find accurate enough. (See below, however, for an important lacuna.)

Devitt's first objection is that Dummett, by construing understanding as capacities to manifest particular sorts of behavior, is

[4] See, e.g., James Higginbotham's "Is Grammar Psychological?" in *How Many Questions: Essays in Honor of Sidney Morgenbesser* Leigh Cauman, Isaac Levi, Charles Parsons, and Robert Schwartz, eds. (Indianapolis: Hackett, 1983): 170–179, esp. p. 178.

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committed to a behaviorist psychology, even though current research suggests that an allegiance to functionalism is preferable. With only a couple of reservations, I find this a valid point. In the first place, Devitt does not distinguish the various kinds of behaviorism someone might be committed to. Dummett is not a behaviorist in the sense that Skinner is, since he is not opposed in principle to mental entities' playing a role in explanatory theories. In the case under discussion, however, Dummett has difficulty in seeing how this can be accomplished without providing some independent test for the presence of the attributed tacit propositional knowledge. He finds one resolution of this problem by taking this knowledge to consist in a practical ability. It is not clear that Dummett rules out all other resolutions; his claim, I presume, is that he has yet to find another one. Devitt is insensitive to the different kinds of behaviorism one might adopt and treats Dummett's as an intellectual prejudice because he does not fully grasp Dummett's rationale for his position.

My second point, related to the first, is that Devitt, because he is not fully aware of the root of Dummett's behaviorism, fails to articulate the challenge that Dummett poses to functionalist accounts. Since Devitt seems to be unaware of this challenge, it is not surprising that he gives no indication how such an account might remain explanatorily adequate while appealing to possession of tacit propositional knowledge, independent tests for which seem to be lacking. Unfortunately, here I have neither the space nor the opportunity to pursue these issues.

Devitt attempts to argue that Dummett's position relies on "an unreconstructed positivist epistemology" (93, 92/3). His objection is to Dummett's requirement that there be "a particular recognizable condition in which the belief expressed by *S* is conclusively justified" (92). I am unclear as to what his objection is. I find evidence for at least three interpretations. According to the first, Devitt is objecting to Dummett's presupposition that there is only one justification for a statement. He writes that "Many different worldly conditions can produce the same sensory stimulation" (92/3) and that "The problem is that there are *indefinitely many* recognizable conditions that could give a belief *some* degree of support" (93). If this is his objection, then he has simply misunderstood Dummett, who has always recognized that there will be many ways to justify a statement, in particular, many ways in which a mathematical statement may be proved, if it can be proved at all (see, e.g., EI §7.2, and WTMII, p. 132). Furthermore, I fail to see how what Devitt takes to be Dummett's requirement commits him to this view.

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On the second interpretation, I find Devitt objecting that Dummett seems to require a notion of absolute, as opposed to relative justification. He writes, "No belief is *conclusively* justified. Each belief is tied loosely to a range of conditions in which, relative to other beliefs, it is justified in varying degrees" (93), and "one condition could give some degree of support to indefinitely many beliefs" (93). I am uncertain why Devitt takes "conclusively" to mean "absolutely" rather than "in accord with the direct means of verification (i.e. those means that correspond to the meaning assigned to the statement by the theory of meaning)" (see, e.g., WTMII, p. 132). Paying attention to Dummett's distinction between direct and indirect means of verification (see, e.g., "The Justification of Deduction" in TOE) might have furnished a more promising interpretation of what he means by "conclusive" verification. Characteristically, Devitt attempts no such thing, but seems content to discuss the issue outside of what may be its proper context. Even if this interpretation of Dummett fails, it cannot be that he requires absolute over relative justification. In discussing the consequences of the evolution of mathematics from an intuitionist's point of view, he raises the possibility that a canonical proof ("conclusive verification") might cease to be acceptable in the future because we come to reject a principle on which it depends (EI 401/2). These observations should make one hesitate before declaring Dummett's commitment to absolute justification.

A third interpretation of Devitt's objection is that he sees no way to pick out a "condition from *S*'s set of possible confirming conditions and make it the verificationist truth conditions of *S* in particular" (93). Dummett's singling out of a particular justificatory condition of *S* is based on his distinction between direct and indirect means of justification. This distinction may be untenable (indeed Dummett considers its questionability in the case of intuitionist mathematics to be the greatest threat to the latter's coherence; see, for example, EI §7.2), but Devitt offers no reason for thinking so (in fact, he apparently fails to see the relevance of this distinction to the issues he is discussing).

Devitt next argues that Dummett's position should be rejected because (a) it "rests on" (99) a descriptional theory of reference, (b) it is incompatible with a causal theory of reference, and (c) the causal theory of reference should be adopted over the descriptional theory. Naturally, Devitt could not argue for (c); so let us assume that it is correct and inquire into the other two premises.

Devitt offers no grounds for (a). He does seem to argue that the description theory "fits nicely with" (94) one of the premises of

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Dummett's argument. But what Devitt must show is that this premise *requires* the description theory. Nothing less will do for Devitt's objection, but I can find no argument to this effect.

I believe that Devitt does not establish (b) and that he is unaware of this because he misses an important qualification in Dummett's position. Devitt's argument is that, according to causal theories of reference,

> A speaker can use a term to refer though almost entirely ignorant about its referent. He may not be able to describe it, recognize it, or know how to track it down. Causal theories have a different view according to which reference is fixed by an appropriate causal link to reality. Judging which part of reality is so linked is a job for the experts, not the essence of what every speaker can do. The opinions of experts depend on their theories, and so opinions may differ and change over time even though reference remains constant (94).

Let us assume that speakers can use a term without actually being able to recognize its referent or track it down by following the appropriate causal chain. Is it clear that this count must against Dummett's position? Dummett's view does not depend solely on speakers' having a capacity *actually* to recognize the conditions of verification of a sentence when confronted with them (for they may never be able to exhibit that capacity because, in practice, it may be impossible for them to place themselves in the required conditions), but depends also on speakers' having a capacity *in principle* to recognize these conditions. Without the move to in-principle recognition, all but finitely many decidable statements of mathematics would have to be considered beyond our comprehension, a position Dummett wishes to avoid. With this in mind, a Dummettian might reply that speakers who use a term may intelligibly be said to have the capacities, in principle, to recognize its referent by tracking it down the appropriate causal chain. The reason for this, it might be claimed, is that experts are able to manifest such capacities and all speakers have the capacity, in principle, if not in practice, to be experts. (We need not ask whether and how the capacity to be an expert, in principle, if not in practice, could be manifested in behavior since we have another independent test for its possession, namely, that the object in question be a normal developed human being. That, it might be plausibly claimed, just is what having that capacity consists in.) Devitt might wish to respond by claiming that experts need not even in principle be able to determine which "part of reality" is linked by the appropriate causal chain to the use of a term; these facts might be necessarily inaccessible to us. Or

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he may wish to grant the in-principle accessibility to us of all facts relevant to determination of reference in accordance with the causal theory, but insist that it is unclear how the experts could manifest through their behavior their ability, in principle, if not in practice, to discover these facts. Finally, he might wonder whether the in-principle/in-practice distinction that Dummett appeals to is intelligible. (I believe that much light on the premises and coherence of Dummett's position would result from an inquiry into this; but I cannot pursue this matter here.) It is not obvious that the causal theory leads to any particular view on these issues. I am, of course, not arguing for any of these positions. My point, simply, is that unless the objections I adduced on Devitt's behalf are defended—and Devitt, of course, fails to do this—, it is unclear that the causal theory of reference must be incompatible with a Dummettian position.

Devitt concludes this section with an examination of the role of the slogan "meaning is use" in Dummett's argument. He claims that the "indubitable fact" behind this slogan does not support the conclusions Dummett draws from it. He writes

> it is what people do with words that makes them mean what they do; in particular, taking a central aspect of meaning, it is what people do that makes it the case that a certain object is the referent of a word. Similarly, it is what a person does that makes it the case that a certain object is his child. It no more follows from the first fact that a person must *be able to recognize the object in question as* the referent [. . .] than it follows from the second fact that a person must *be able to recognize the object in question as* his child (95).

Here Devitt again neglects the Dummettian addition that speakers have the capacities to make, *in principle,* the relevant recognition. So Devitt's parental analogy need not count against the Dummettian, for one does feel tempted to say that parents could, in principle, engage in an inquiry that would lead them to recognize their children as their own. But these remarks assume that Devitt's analogy is valid—a dubious assumption, at best, since it is not just *any* action of the speaker that determines a referent for his words but, rather, at least in part, his thoughts involving that word, i.e., psychological or intentional acts the presence of which or the consequences of which should be accessible to consciousness in a way or to a degree that the consequences of our physical actions are not. In addition, Devitt does not try very hard to find a justification of "meaning is use" that would warrant the uses to which Dummett puts it. In particular, he entirely fails to mention here, as else-

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where, Dummett's great concern about the possible vacuity and circularity of explanations that involve attributing tacit knowledge of meaning to speakers.

IV

I will conclude here, though I have not touched upon Devitt's brief articulation of his own views, nor even upon all the details of Devitt's reconstruction of, or his objections to, Dummett's argument against realism. In sum, there are many interesting issues to be raised and discussed concerning Dummett's philosophy, some of which I signaled above. Unfortunately, I find that Devitt touches upon few of these and that those he does are dealt with in an unsatisfying manner.

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Harvard University

NOTES AND NEWS

The editors report with sorrow the death of Herbert Bohnert on March 12, 1984, in Fortuna, California. Professor Bohnert was born in 1918, received a B.A. from the University of Chicago and a Ph.D. from the University of Pennsylvania. He held positions as a mathematician and scientist in the computer field from 1951 to 1968. He joined Michigan State University as a professor in 1968 and retired in 1982.

The editors regret to report the death of Michel Foucault, French philosopher, psychologist, and historian, on June 25, in Paris. Since 1970 Foucault had held the chair in History of Thought at the Collège de France in Paris. The third volume of his *History of Sexuality* had recently been published. At the time of his death he was 57 years old.

The Third Biennial Meeting of the Society for Philosophy and Technology will be at the Technical University of Twenthe, The Netherlands, from August 12–16, 1985, on a general theme of technology and responsibility. Symposia are being planned on the integration of technology and society, philosophical anthropology and technology, methodological issues, technology and work, autonomy of technology, engineering ethics, and religion and technology. Papers on other subjects and suggestions for additional symposia are welcome. The official language of the conference is English, but papers written in German will be accepted with English summaries. The deadline for receiving papers for consideration is November 30, 1984. All correspondence should be addressed to the President, Alex C. Michalos, Philosophy Department, University of Guelph, Ontario, Canada, N1G 2W1.

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An international conference on "Issues in Nuclear Deterrence" will be held at the Inter-university Centre of Postgraduate Studies, Dubrovnik, Yugoslavia, June 1–4, 1985. Main topics will include the threat of using nuclear weapons; ethical responsibilities and obligations of military and political leaders as well as ethical responsibilities and obligations of civilians; analysis of mass self-deception about the nuclear threat; nuclear buildup as an economic weapon; whether or not claims of deterrence in foreign affairs can be confirmed; and how the notion of deterrence in nuclear-weapons-use strategies differs from that of other areas (e.g., deterrence in punishment and learning theory). Inquiries from persons interested in participating in the conference can be made to any of the following directors: Trudy Govier, Interdisciplinary Studies, University of Calgary, Alberta, T2N 1N4; Steven C. Patten, Department of Philosophy, University of Lethbridge, Alberta, T1K 3M4; and Richard Werner, Department of Philosophy, Hamilton College, Clinton, N.Y. 13323.

Applications are invited for Junior Research Fellowships in Philosophy at King's College, Cambridge. The Fellowships will commence on 1 October 1985. The normal tenure is four years, ending on 30 September 1989. Men and women graduates of any university are eligible provided that they have not completed more than about five years of post-graduate research by October 1984. Besides the stipend, an unmarried Fellow is provided with accommodation in College if he or she wishes. Applications will need to be made by 12 October 1984. Further details can be obtained from the Provost, King's College, Cambridge CB2 1ST.

The Editorial Board of the *Philosophical Quarterly* announces an annual essay prize. The topic for 1985 is "Philosophy of Mind." The closing date for submissions is January 31, 1985. Further information may be obtained from: The Executive Editor, University of St. Andrews, St. Andrews, Scotland, Fife, KY16 9AL.

The fourth international WHIM humor conference will be held on the campus of Arizona State University from March 29 to April 1, 1985. The deadline for proposals, on any aspect of "Humor across the Disciplines" is December 1, 1984. More information may be obtained from Don and Alleen Nilsen, chairs, WHIM Conferences, English Department, Arizona State University, Tempe 85287.

The Bertrand Russell Society announces a call for papers to be presented at its meeting at the Eastern Division of the American Philosophical Association in December 1985. The submission deadline is May 15, 1985, and the papers should be sent to David E. Johnson, Chairman, Philosopher's Committee, The Bertrand Russell Society, Sampson Hall, U.S. Naval Academy, Annapolis, Md. 21402.

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The International Society for Chinese Philosophy is sponsoring its Fourth Biennial International Congress in Chinese Philosophy, on the theme "Human Nature and Culture," scheduled to be held July 15–18, 1985, at SUNY at Stony Brook. Those who wish to present papers or organize panels are advised to submit one-page abstracts to Lik Kuen Tong, Philosophy Department, Fairfield University, Fairfield, Conn. 06430, no later than November 30, 1984.

A national conference entitled "Creativity and Science" will be held in Honolulu, March 23/24, 1985. Keynote speakers include: Leon Edel (English, Hawaii) on the nature of psychological evidence; Lenn Goodman (Philosophy, Hawaii) on why machines cannot do science; Harold Morowitz (Molecular Biophysics and Biochemistry, Yale) on pantheism and modern science; George Wald (Biology, Harvard) on life and mind in the universe. Papers are invited from scientists and humanists on all aspects of the conference theme. Funds are available on a limited basis for travel support and expenses of visiting scholars. Further information may be obtained from L. E. Goodman, Department of Philosophy, University of Hawaii, 2530 Dole Street, Honolulu 96822.

The Center for Dewey Studies and the John Dewey Foundation sponsor academic-year fellowships for research that seeks to illuminate and critically assess aspects of the philosophy of John Dewey. Awards are available to senior scholars in various disciplines interested in exploring issues and problems related to John Dewey's philosophical concerns. Application forms are available from The Center for Dewey Studies, Southern Illinois University, Carbondale, Illinois 62901.

The Eleventh Annual Meeting of the Southeastern American Society for Eighteenth Century Studies will be held at Athens, Georgia, on March 7–9, 1985, with the University of Georgia as host. Proposals for papers on any eighteenth-century topic are solicited. Prospective participants are encouraged to organize sessions and to secure other speakers, but individual proposals will also be accepted. All proposals, papers, and inquiries should be addressed to the Program Chairman: Carl R. Kropf, Department of English, Georgia State University, Atlanta 30303.

The seventy-sixth annual meeting of the Southern Society for Philosophy and Psychology was held April 19–21, 1984, in Columbia, South Carolina, hosted by the University of South Carolina. Joint sessions included the Presidential Address by Arthur J. Riopelle, "Annual Models in Research: Questions Raised by Data from Nutrition Studies," and an invited address by Roger S. Sperry, "Cognitive Role of Belief: Implications of the New Mentalism." Griffith Award winners were David N. James, Longwood College, in philosophy, and Douglas E. Landon, U.S. Army Aeromedical Research Laboratory, in psychology. Officers elected for 1984/85 were Rem

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B. Edwards, University of Tennessee, President; M. Carr Payne, Georgia Institute of Technology, President-Elect; Stephen F. Davis, Secretary, and Council Members James L. Pate, Georgia State University, and Jimmy W. Dye, Northern Illinois University. Continuing officers are Arthur J. Riopelle, Louisiana State University, Past-President; Nancy D. Simco, Memphis State University, Treasurer; and Council Members Mary Anne Baker, University of Indiana Southeast, Michael Hodges, Vanderbilt University, James F. Harris, Jr., College of William and Mary, and Norman R. Remley, Texas Christian University. The 1985 meeting of the Society will be held April 4–6, in New Orleans, Louisiana.

"Women and Moral Theory," a multi-disciplinary conference examining feminist challenges to traditional moral philosophy, will be held March 22–24, 1985, at SUNY Stony Brook. Speakers include: Carol Gilligan, keynote address; Kathryn Pyne Addelson, Annette Baier, Sheyla Ben-Habib, James Fishkin, Carol Gould, Sandra Harding, Virginia Held, Mary Feinsod Katzenstein, Richard Miller, Sara Ruddick, George Sher, and Carolyn Whitbeck. Further information may be obtained from: Eva Feder Kittay, Department of Philosophy, SUNY Stony Brook, L.I., N.Y. 11794, or Diana T. Meyers, Department of Government, Cornell University, Ithaca, N.Y. 14853.

The Social Philosophy and Policy Center of Bowling Green State University will hold a conference entitled "Morals and Markets" Sept. 21, 1984. Original papers will be presented by Jules Coleman (Arizona), Alexander Rosenberg (Syracuse), and Amartya Sen (Oxford). Their commentators will be Donald Regan (Law, Michigan), Hal Varian (Economics, Michigan), and Allan Gibbard (Philosophy, Michigan). Further information may be obtained from John Ahrens, Social Philosophy and Policy Center, Bowling Green State University, Ohio 43403.

The philosophy department of the University of Calgary is pleased to announce the appointment of Dennis E. McKerlie (Oxford) as assistant professor, effective July 1984, and of Michael M. Burgess, Ronald R. Clark, and James O. Young as sessional instructors. John D. Collier and M. Alison Wylie have been awarded postdoctoral fellowships, which they will hold in the department. Kai E. Nielsen has been appointed department head, effective July 1984; he succeeds Brian F. Chellas, who has been appointed dean of the Faculty of Humanities.

The Department of Philosophy of California State University, Northridge, is pleased to announce the appointment of Richard Rodewald as Assistant Professor.

The Department of Philosophy at Columbia University is pleased to announce that the 1984 John Dewey lectures will be delivered on September 17, 18, and 19, by Amartya Sen of All Souls College, Oxford, under the title

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"Freedom and Well-being." Previous John Dewey lecturers have been W. V. Quine (1968), A. J. Ayer (1970), Ernest Nagel (1977), and John Rawls (1980).

The Philosophy Department of the University of Guelph is pleased to announce that Alex C. Michalos has been awarded the Secretary of State's Prize for Excellence in Interdisciplinary Research in Canadian Studies, for his *North American Social Report,* a comparative study of the quality of life in Canada and the USA from 1964 to 1974. The five volumes of this treatise were published by D. Reidel as *Foundations, Population and Health* (1980), *Crime, Justice and Politics* (1980), *Science, Education and Recreation* (1981), *Environment, Transportation and Housing* (1981), and *Economics, Religion and Morality* (1982).

Harvard Law School offers fellowships to college and university teachers in the social sciences and humanities to enable them to study fundamental techniques, concepts, and aims of law, so that, in their teaching and research, they will be better able to use legal materials and legal insights which are relevant to their own disciplines. Further information may be obtained from the Chairman, Committee on Liberal Arts Fellowships in Law, Harvard Law School, Cambridge, Mass. 02138.

The Department of Philosophy at the University of Illinois at Chicago is pleased to announce that David Copp (Simon Fraser) and Mark Wilson (Univ. of California, San Diego) will be joining the department as associate professors in the Fall 1984.

The Department of Philosophy of McMaster University is pleased to announce the appointment of Wilfrid Waluchow as Assistant Professor of Philosophy.

The Department of Philosophy of the University of Miami, Coral Gables, Florida, is pleased to announce the appointment of Harvey Siegel (Harvard) as Associate Professor of Philosophy. Howard Pospesel (North Carolina) has been appointed chairman of the department.

Montclair State College's Institute for the Advancement of Philosophy for Children is pleased to announce that two children's philosophy organizations, the International Council for Elementary and Secondary School Philosophy and the Center for the Improvement of Reasoning in Early Childhood, have joined as new members of the Institute. The Institute is also pleased to announce that it will offer one-, three- and six-month internships, as well as one-year fellowships for those interested in observing ongoing Philosophy for Children programs or in doing research or writing about philosophy for children. Further information may be obtained from Institute for the Advancement of Philosophy for Children, Montclair State College, Upper Montclair, N.J. 07043.

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On Oct. 24 and 25, Montefiore Medical Center will sponsor a double-focus symposium: Aging and the Dementias, and Aging in the Twenty-first Century, to be held at the Rockefeller University. The first day will be devoted to the clinical aspects of aging, with an emphasis on dementia, and the second to the social, economic, and political ramifications of the aging process. Further information can be obtained from: The Montefiore Centennial Series, Montefiore Medical Center, 111 East 210, NYC 10467.

The Department of Philosophy of the New School Graduate Faculty announces its 1984 Hannah Arendt Memorial Symposium, on "Political Discourse and Totalitarianism," to be held November 26 and 27. Speakers will include: Gehard Grohs (Mainz University, West Germany), Agnes Heller (La Trobe University, Australia), Jürgen Link (Bochum University, West Germany), Jean-François Lyotard (University of Paris-Vincennes, France). Further information may be obtained from the Department of Philosophy, New School for Social Research, 65 Fifth Avenue, NYC 10003.

The Philosophy Ph.D. Program of the CUNY Graduate Center is happy to announce the following visitors for 1984/85: fall term, Annette Baier of the University of Pittsburgh, spring term, Stephen Schiffer of the University of Arizona.

The Department of Philosophy, Oberlin College, is pleased to announce the appointment of the following visiting professors: Simon Blackburn, Pembroke College, Oxford (Fall 1984); Anthony Kenny, Balliol College, Oxford (Winter 1985); and Michael Stocker, LaTrobe University, Australia (Spring and Fall, 1985 and Winter 1986). Alfred F. MacKay, Professor of Philosophy, has been appointed Dean of the College of Arts and Sciences.

The Department of History and Philosophy of Science at the University of Pittsburgh is pleased to announce the appointment of John Norton as visiting assistant professor.

The Stanford Humanities Center will be offering five to seven external fellowships for 1985/86 intended for scholars and teachers in the humanities who would be interested in spending the academic year at Stanford. The fellowships are primarily intended to enable fellows to pursue their own research and writing; however, recipients are also expected to devote about one-sixth of their time to teaching or in some other way contributing to intellectual life at Stanford. The deadline for application is December 14, 1984. Application materials and fuller information regarding eligibility, stipends, and selection criteria may be obtained by writing Morton Sosna, Associate Director, Stanford Humanities Center, Mariposa House, Stanford University, Calif. 94305.

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A symposium entitled "Reading Nietzsche" will be held at the University of Texas at Austin from February 21-23, 1985. Contributors include: Arthur Danto, Bernd Magnus, Christopher Middleton, Alexander Nehemas, Richard Schacht, Ivan Soll. Further information may be obtained from Robert C. Solomon or Kathleen Higgens, Dept. of Philosophy, University of Texas, Austin 78712.

NEW BOOKS IN PAPERBACK

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ADORNO, THEODOR W.: *Prisms.* Cambridge, Mass.: MIT Press, 1983. 272 p. $6.95.

AGUIRRE, ANTONIO F.: *Die Phaenomonologie Husserls im Licht ihrer gegenwartigen Interpretation und Kritik.* Darmstadt: Wissenschaftliche Buchgesellschaft, 1982. vii, 173 p. DM 41.

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BADER, FRANZ: *Descartes' Erste Philosophie: Die Systematik des methodische Zweifels.* Bonn: Bouvier, 1982. 330 p. DM 66.

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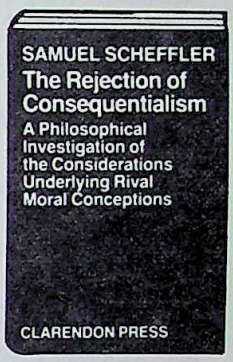
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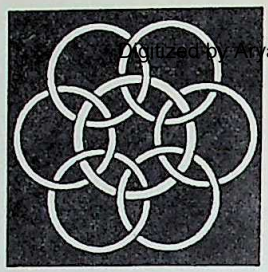

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THE JOURNAL OF PHILOSOPHY
VOLUME LXXXI, NO. 10, OCTOBER 1984

SKEPTICISM WITHOUT INDUBITABILITY*

IN *Philosophy and the Mirror of Nature* Richard Rorty traces the emergence of "foundationalist epistemology . . . as the paradigm of philosophy."[1] According to Rorty, a key step in this development was Descartes's un-Aristotelian construal of "sensory grasp of particulars" as "mental" (54). The resulting conception of the mind as "an inner arena with its inner observer" "permitted" the seventeenth century "to pose the problem of the veil of ideas, the problem which made epistemology central to philosophy" (51). Thus Descartes, by "carving out inner space," "simultaneously made possible veil-of-ideas skepticism and a discipline devoted to circumventing such skepticism" (140).

In seeking to account for Descartes's fateful introduction of an account of sense perception focused on ideas of sense, Rorty tentatively appeals to an apparently idiosyncratic Cartesian preoccupation with "indubitability" (55). Although Descartes himself sought the foundations of knowledge in distinct ideas of reason rather than in sensory particulars, the move to empiricistic foundationalism was readily accomplished by Locke, on Rorty's account. On the Cartesian model, adapted by Locke, the Eye of the Mind is restricted to surveying the ideas or representations in inner space, "hoping to find some mark which will testify to their fidelity" to extra-mental reality (45).

Rorty is surely right that a strong distinction or contrast between sensations or "ideas of sense," on the one hand, and external physical things on the other hand, emerges as a central doctrine in seventeenth-century philosophy. Further, the distinction is, as he indicates, closely connected with both the modern mind-body

*To be presented in an APA symposium on Skepticism, December 28, 1984. Barry Stroud will be co-symposiast, and Robert Fogelin will comment; see this JOURNAL, this issue, 545–551 and 552, respectively, for their contributions.

[1] Princeton, N.J.: University Press, 1979, p. 59. Subsequent page references are given in the text.

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problem and certain sorts of skeptical reasoning. It is also true that both Descartes and Locke speak of knowledge as requiring a "foundation" in indubitable ideas. Nevertheless, there is a crucial element missing from Rorty's account of seventeenth-century concerns: an element that is also omitted or slighted by many other critics who either look back on the period from the dubious vantage of twentieth-century sense-data theories, or look forward (so to speak) from the standpoint of ancient skepticism. I have in mind the fact that the seventeenth-century contrast between ideas of sense and physical things at least partly derives from the conception of material reality provided by mechanistic science, and the disparity between this conception of reality and the seeming givens of ordinary perceptual experience.[2]

I

Underlying seventeenth-century accounts of perception is the metaphysical view that "real and physical" qualities include just extension, figure, motion, and (for some) impenetrability or solidity (together with structural qualities such as texture, which result from combining extended particles in various ways). Proceeding from this conception of material reality, philosophers of the period espouse a broad theory of perception that includes the following basic tenets:

(1) In ordinary sense experience certain entities arise "in us" or "in the mind." These entities are variously referred to as "ideas of sense," "sensations," "images," "phantasms," or (rarely) "species."
(2) These sensory ideas (as I shall call them) are formed in the mind as the result of mechanistic processes, involving (characteristically) material things external to our bodies, and the sensory systems of our bodies themselves.
(3) Sensory ideas correlate with discrete sensible qualities, like blue, soft, putrid. In Locke's words, they "enter by the senses simple and unmixed," even though the qualities that produce them are "united and blended" in the things themselves.
(4) Whole classes of the sensory ideas formed in our minds—ideas of colors and tastes, for instance—are completely "unlike" the material things we are ordinarily said to perceive, or any qualities really in the things. This tenet is universally regarded as contrary to common sense.
(5) We do apprehend real qualities of material things, through our representations of size, shape, and motion. Not all philo-

[2] Of course numerous historians have stressed this point, but its importance continues to be overlooked, particularly by epistemologically oriented commentators.

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sophers clearly indicate whether our representations of the latter qualities are always sensory ideas, and there is not much unanimity among those who do take positions on this point.

The theory constituted by these five tenets is vigorously propounded by Hobbes, Malebranche, and Boyle, as well as Descartes and Locke. They commonly ascribe to the conception of physical reality that underlies this view of perception an a priori or essentialist grounding in "the concept of body." Descartes, Boyle, and others also stress the *explanatory success* of strictly mechanistic concepts in accounting for perception, as well as the other phenomena of nature.

The relations between this general account of perception and skeptical concerns are complex. Here I wish to distinguish two separate connections. In the next section I focus on the notion, implicit in tenet (4), that human beings in general are systematically and literally constantly deceived in ordinary sense experience. Seventeenth-century philosophers employ skeptical arguments in aid of this conviction: to this extent the arguments function instrumentally, and depend on a confident background conception of the true nature of material things. In the third section I consider more closely some relations among the characteristic seventeenth-century account of sense perception, the "veil-of-perception" position (which has its proponents today), and non-instrumental skepticism.

Approached from the perspective of theory of nature and the resulting conception of perceptual experience, the problem of sensory contact with the external world that arises in seventeenth-century thought appears both more readily intelligible and less tightly dependent on preoccupations with justification and certainty than is often supposed. Accordingly, the problem does not automatically vanish if we follow Rorty in rejecting, as anachronistic or artificial, attempts to "ground" empirical knowledge claims in an indubitable sensory given.

II

A typical statement of the distinction between ideas of sense and physical qualities occurs in the following passage from Malebranche:

> In order . . . to judge soundly about light and colors, as well as all the other sensible qualities, one must distinguish with care the sensation [*sentiment*] of color from the movement of the optic nerve, and recognize by reason, that movements and impulsions are the properties of bodies, and that hence they can be encountered in the objects, and in the organs of our senses; but light, and the colors that we see, are

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modifications of the soul quite different from the others, and of which we also have quite different ideas.[3]

Of course seventeenth-century writers often acknowledge interpretations of a statement like "the violet is blue" on which this statement comes out literally true: in saying that the violet is blue we may be predicating of it a certain superficial microstructure, or a "power" deriving from such a surface structure to affect sentient beings with a particular sort of sensation. Nevertheless they steadfastly maintain that such qualities in objects are utterly different from—fail to "resemble"—sensory appearances. Thus, they *do* deny that colors and so forth are in objects in the way that we normally take them to be—as real, discrete, irreducible qualities, manifest in sense experiences, and on a footing with size and shape. Contrary to Malebranche's admonition, we do not normally judge soundly about colors, smells, tastes, sounds, and the like, but (as Locke says) "by mistake attribute" reality to them. Indeed we are peculiarly obstinate in our error: "Men are hardly to be brought to think, that *Sweetness and Whiteness are not really in Manna*."[4] The same point is expressed, in quite similar terms, by Descartes, Malebranche, Hobbes, and Boyle.

This sort of universal "deception" is of much more than passing concern. The widespread human error about the objectivity of sensible qualities is generally felt to require some kind of explanation, and even to raise problems of theodicy. Descartes elaborately traces such errors to habits arising in infancy, and to a tendency simply to conflate mere sensations—intended to alert us to things good or bad in the environment—with our "clear" perceptions of physically real qualities.[5] Malebranche, who develops a quite complex analysis of perceptual judgment and error, locates a source of the basic problem in the fall of Adam.[6] Hobbes offers a *physical* explanation of our tendency to project sensations on things in terms of the "endeavor outward" that he takes to be characteristic of sensory processes.[7] (Some of these philosophers also discuss ways of overcoming such "error.")

The claim that the senses constantly—not just episodically—lead us into error about physical reality provides an important perspective for understanding the stress on skeptical arguments in seventeenth-century work. In Descartes and Malebranche, in particular, such arguments are overtly used to shake our faith in the senses, as purveyors of objective truth. "Detachment from sense" is of course

[3] *Search for Truth*, I, xii, 5.
[4] *Essay concerning Human Understanding*, II, viii, 14, 18.
[5] *Principles of Philosophy*, I, 67/8.
[6] *Search*, I, v, 1 and 2; I, xii, 2.
[7] Cf. *Leviathan*, I, i.

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not a professed aim of empiricists like Hobbes or Locke. Still Locke and other writers draw on the relativity considerations associated with traditional skepticism to underscore the subjectivity of certain broad classes of sensory states, to which their theory of reality independently commits them.

From this perspective, relativity arguments, Descartes's dreaming argument, and other attacks on the senses, appear to be propounded for a decent dogmatic purpose. Their role is neither to convert us to an equinanimous agnosticism, nor to focus our attention on our indubitable "inner sensory representations" with the aim of building from them to well-founded objective knowledge claims. They are intended to prepare us to accept the austere "new" truths about what the world around us is really like in the face of our natural resistance.

III

The tenets of the view of sense perception I outlined above do not explicitly include either the claim that sensory ideas are states of "mental substances" or the claim that the mind perceives (or immediately perceives) only its own ideas. In fact, historically, this conception of sense perception is not invariably accompanied by the latter claims. Hobbes, for example, definitely maintains that we perceive physical objects, not the sensory "phantasms" they produce in us.[8] Further, he holds that all substances are bodies, and explicitly identifies phantasms with motions in the sensory system. Despite this fact, the account of perception that arises from the mechanistic picture of nature does help illuminate—more, I think, than any preoccupation with indubitability or epistemological foundations—the systematic origins of the veil-of-perception position.

It *is*, as we've seen, an explicit feature of the widely shared seventeenth-century view that we err in thinking of colors-as-experienced (for example) as real qualities of bodies. What are they then? Hobbes's bold identification of sensory experiences with material states of the sentient organism was a far from unproblematic move—as recent decades of debate of the materialist identity thesis have surely made clear. It seems understandable enough that Descartes and some of his successors should conclude that colors and other "mere sensations" are no more to be identified with states of the nervous system than with "external" physical qualities—and see no alternative to construing them as merely mental entities. Further, while Descartes does connect this interpretation of the status of sensations with indubitability in the *Meditations*, con-

[8] *Concerning Body*, xxv, 3, 10. Hobbes does sometimes speak of phantasms as "proper objects of the senses."

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sideration of the early parts of the *Treatise on Light* and the *Dioptrics* (for instance) shows that he by no means invariably does so. (Malebranche is merely restating Cartesian assumptions when he remarks that the true understanding of the difference between mind and body was achieved only "in the last few years," when it came to be appreciated that "sensible qualities" *are not contained in the idea of matter.*)[9]

The view that we do not perceive—or immediately perceive—physical objects is sometimes said to be a "presupposition" or a logical consequence of the seventeenth-century distinction between sensory ideas of color and the like, on the one hand, and the real qualities of bodies on the other hand.[10] I believe that this is an overstatement: one can consistently give a Hobbesian account of perceptual experience in terms of phantasms, without taking the step of holding that we *perceive* phantasms. But there *is* a fairly simple path to the view that what the Eye of the Mind perceives are its own ideas, from the account of perception summarized above. The key notion is that when we think we are looking at, listening to, touching (etc.) the world we after all do perceive colors, sounds, warmth, and cold, etc. But colors, sounds, warmth, and cold, etc. are merely ideas or sensations in the mind. Therefore, what we perceive in these cases, at least, are (properly) ideas. This, I take it, is the viewpoint behind the following classic Cartesian statement of the veil-of-perception position:

> . . . [B]esides the extension, figures, and motion of bodies, I also sensed in them hardness, and heat, and other tactile qualities; and further light, and colors, and odors, and tastes, and sounds, from the variety of which I distinguished heaven, earth, sea, and other bodies from each other. And surely it was not without reason, on account of the ideas of all these qualities which offered themselves to my thought, and which alone I properly and immediately sensed, I thought I sensed certain things completely different from my thought, namely bodies from which these ideas proceeded.[11]

Admittedly, the reasoning sketched so far does not explicitly cover figure, motion, and other qualities that (on the mechanistic hypothesis) "are not otherwise sensed or understood by us than they are, or at least can be in objects."[12] There are various considerations that would tend to support the inclusion of perceptions of such qualities in the "veil" theory: the consideration, for instance,

[9] *Search*, Preface [N. Malebranche, *Oeuvres*, I. ed. G. Rodis-Lewis (Paris: Gallimard, 1979)], p. 13.
[10] Cf. J. L. Mackie, *Problems from Locke* (New York: Oxford, 1976), p. 7.
[11] Meditation VI [*Oeuvres de Descartes*, ed. C. Adam and P. Tannery (Paris: Cerf, 1887–1913), vol. VII], pp. 74/5.
[12] Descartes, *Principles*, I, 70.

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that sensibly experienced shapes and so on are pervaded by the qualities regarded as thoroughly subjective, like color. As mentioned under tenet (5), however, this is a textually complex issue; and I cannot fully deal with it here.

The account so far offered indicates why—apart from preoccupation with indubitability or foundationalism—Descartes and his successors would tend to treat sensory grasp of particulars as falling within the realm of the mental; why they might tend to consider our immediate perceptions as failing to provide *direct* cognitive contact with physical reality; and why they would seek to instill doubt of the senses. The question that remains to be considered is whether this perspective on the veil-of-perception position suggests any connection with skepticism of a noninstrumental sort.

Now, I think in fact concern with "circumventing" veil-of-perception skepticism—or seeking internal marks of "fidelity" in inner representations of sense—is far from rampant in seventeenth-century philosophy. (Incidentally, such concern is arguably more noticeable in Descartes and Malebranche than in Locke.) This observation fits with the tendencies to untroubled perceptual realism which have often been discerned in the period and with the notion that the veil-of-perception position is correlative to a positive theory of nature. Nevertheless, it seems to me that the position does permit—even, ultimately, support—a very perplexed attitude toward our sensory contact with the world, whether or not seventeenth-century theorists themselves exhibit serious perplexity about this problem. Berkeley put his finger on the crucial point when he objected that the materialists, for all their emphasis upon explanatory success, in fact could offer no intelligible explanation of how sensory ideas are produced by matter.[13]

Suppose that someone who accepts the proposition that the immediate objects of perception are mental, requests an account of how exactly the sensory particulars that he immediately perceives are connected with, serve to relate him cognitively to, real things out there. The cheerful Boylean or Lockean answer—heard again in recent years[14]—is that the things *produce* the ideas via our sensory systems; Descartes, in his more searching moments, holds that changes in the brain serve as signs to the mind, giving it "occasion" to generate sensations.[15] The latter view leaves unilluminated the central step of the mind's receiving signs from the brain, and to this extent seems dismissible as a vague metaphor. The simpler language of causal production seems more plausible, more com-

[13] See, e.g., *Principles of Human Knowledge*, I, 50.
[14] Cf. Frank Jackson, *Perception* (New York: Cambridge, 1977), p. 126 and *passim*.
[15] Cf. *Notes against a Program*, AT VIII-2, 358/9.

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monsensical, even scientific. However, I am inclined to think that this appearance is deceptive.

According to proponents of the veil-of-perception view our sensations are remotely caused or explained by the physical objects that we indirectly perceive; proximately they are caused or explained by material changes in our brains. Against the objection that the position leads to skepticism because of a problem in principle about postulating "unobserved causes," recent advocates have argued (with some reference to scientific procedure) that such postulation is fully justified when it is explanatorily effective.[16] But this brings us back to the question whether an intelligible explanation of sensation in terms of (say) brain states is in fact available or (at least) imaginable. In the seventeenth century the question took the form of consternation about how body acts on mind, as well as indications that inference from sensory ideas to their putative explanatory causes is peculiarly problematic. Today we find objections to the notion of psychophysical laws. Although these objections are not usually related specifically to perception, they do serve to raise doubts about the ultimate intelligibility of the veil-of-perception position. (Can we, for instance, tolerate the possibility that *all our immediate objects of perception* are "nomological danglers"?[17]) There are also more specific problems. For instance, considered from the physical side, the causal chain said to result in ideas of sense seems literally to go nowhere. The qualia we perceive (ideas of sense) by hypothesis are not qualities of physical things; one may presumably infer that they are not locatable in physical space. If so, it follows at least that a Humean conception of causal relations including a contiguity condition cannot be fulfilled.

Such reasons for questioning the intelligibility of the relation between our immediate objects of perception and material things, as portrayed by the veil-of-perception theory, are no more tied to preoccupation with foundationalist epistemology than the position itself is strictly the offspring of a chimerical quest for certainty. And I think they are sufficient for skepticism of a sort.

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[16] Cf. Mackie, *op. cit.*, ch. 2, and 7; Jackson, *op. cit.*, ch. 6.

[17] J. J. C. Smart argues for this conception of "irreducibly psychic" sensations in "Sensations and Brain Processes," *Philosophical Review*, LXVIII, 2 (April 1959): 141–156.

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SKEPTICISM AND THE POSSIBILITY OF KNOWLEDGE*

SKEPTICISM in recent and current philosophy represents a certain threat or challenge in the theory of knowledge. What is that threat? How serious is it? How, if at all, can it be met? What are the consequences if it cannot be met?

I obviously do not have time to go into all these questions, or into any of them thoroughly. I can only sketch a point of view in the hope of provoking some discussion.

The first question is clearly the place to start. I believe the true nature of the skeptical threat is still not properly understood, nor are the consequences of its not being met. That is one reason we have tended to give inadequate answers to the other questions. It is still widely felt that skepticism is not really worth taking seriously, so it hardly matters whether the challenge can be met or not. That kind of reaction seems to me to rest on a philosophical misconception.

Many would dismiss skepticism and defend not taking it seriously on the grounds that it is not a doctrine or theory any sensible person would contemplate adopting as the truth about our position in the world. It seems to them frivolous or perverse to concentrate on a view that is not even a conceivable candidate in the competition for the true or best theory as to how things are. I would grant—indeed insist—that philosophical skepticism is not something we should seriously consider adopting or accepting (whatever that means). But does that mean that it is silly to worry about skepticism? I think it does not. A line of thinking can be of deep significance and great importance in philosophy even if we never contemplate accepting a "theory" that claims to express it.

One reason that is so is that philosophy thrives on paradox, absurdity, dilemma, and difficulty. There are often what look like good arguments for surprising or outrageous conclusions. Taking the paradoxical reasonings seriously and re-examining the assumptions they rest on can be important and fruitful when there is no question at all of our ever contemplating adopting a "theory" or doctrine embodying the absurd conclusion.

The point is clearest in the case of antinomies—explicit contradictions. We know we cannot believe the conclusion; it couldn't possibly be true. To take The Liar, or Russell's paradox, seriously is not to hold open even the remote option of believing that some-

*To be presented in an APA symposium on Skepticism, December 28, 1984. Margaret D. Wilson will be co-symposiast, and Robert Fogelin will comment; see this JOURNAL, this issue, 537–544 and 552, respectively, for their contributions.

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one who says he is lying speaks both truly and falsely, or that there is a set that both is and is not a member of itself. Such "theories" would be worse than outrageous as things to believe, but that in no way diminishes the need to take seriously the reasoning that leads to them.

The same is true even when the conclusion of the paradoxical or surprising reasoning falls short of explicit contradiction. The Eleatic doctrine that nothing moves, for example, need not be in any remote sense a live intellectual option for us in order for us to be rightly challenged, overwhelmed, perhaps even stumped, by Zeno's argument that Achilles can never overtake the Tortoise. The mere idea of something's being true at a time can seem to generate the absurd result that there is never any real alternative to what happens, that things are fated to happen as they do. We can be impelled to investigate that line of reasoning without thinking that otherwise we would have to adopt the "theory" that we have no control over what we do or what happens to us. Again, it seems undeniable that adding one more molecule to a table would not turn it into a non-table, any more than pulling one hair from a bushy head would make it bald. The discomfort I feel in the thought that an exactly similar step can be taken again, and again, does not show that I in any way consider accepting a "theory" according to which there could be a table the size and shape of the earth, or that a bushy head and a bald head are the same sort of thing.

Those modern philosophers most closely connected to the skeptical tradition and most impressed by skeptical reasoning—Descartes, Hume, and Russell, for example—do not hold that believing the conclusions of that reasoning is a real option for us. The ancient skeptics themselves seem not to have accepted, or to have contemplated accepting or declaring the truth of any "theory" either. They were highly anti-theoretical philosophers, and their strictures would have extended to any theoretical pronouncements put into their own mouths by their opponents as well. But none of that shows that skeptical ideas were not worth taking seriously or were not of great philosophical importance.

The importance of skepticism came always from the uses to which its ideas were put—different uses at different times. It is now widely understood to represent a certain threat or challenge in the theory of knowledge. That is not to say that everything in epistemology as we think of it today, or even in that challenge, can be traced back to the skeptical tradition alone. Exactly which skeptical ideas were important in defining the modern philosophical concern with human knowledge, how and to what extent they were

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used, and to what effect—all these are intriguing historical questions. Clearly, it is complicated. The role of sense-perception in our knowledge of the world became an important issue even for those apparently untouched by skepticism—by those in the atomist tradition, for example, from Galileo to Boyle and Locke, as well as by Descartes himself in his studies of optics and the physiology of perception. I want to concentrate for the moment on the problem or challenge itself. I think that, whatever its historical source, it has come to define, or perhaps even create, the philosophical concern with our knowledge of the world.

What do we want from a philosophical theory of knowledge? What is it supposed to do? It seems that we simply want to understand how we get the knowledge we have—to explain how it is possible. But I don't think that is enough to uniquely identify the philosophical problem.

Take what is usually called in philosophy "our knowledge of the world around us." Now it seems obvious, without any philosophical preconceptions, that there are countless ways of coming to know something about the world around us. I can find out that there is a bus-drivers' strike in Rome, for example, by waiting in vain for a bus or by reading a newspaper or by getting a letter from a friend. How many different ways of finding out is that? Is reading a newspaper only one way, or possibly many? Is reading it in the *New York Times* a different way of finding it out from reading it in the *New York Post*? It seems hopeless to try counting. Obviously we do not just want a list of sources. What we seek in philosophy is not just anything that is true about how we get knowledge of the world around us.

The philosophical interest in knowledge is general, and in at least two different ways. We are interested in all of our knowledge of the world taken all together, or in some domain characterized in general terms. To ask only how we come to know some things in the domain, given that we already know certain other things in it, is not to ask about all knowledge of that kind in general. And we don't just want a heterogeneous list of ways of coming to know. We want to find a single way, or a small number of very general "ways of knowing." To explain how they work will be to explain, in general, how knowledge of the kind in question is possible.

Is that enough, then, to identify what we are interested in in the philosophy of knowledge? I don't think so. Suppose we eventually establish contact with some beings elsewhere in space. We receive some regular signals, we send back similar messages, and eventually find ourselves communicating with something somewhere. We take the opportunity to find out about them. We ask them where

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they are, what it is like there, what they are like, how they send out their signals, how they receive ours, and so on. Suppose they do the same with us. One day there appears on our receiving screen the question "How do you come to know of the things around you?" We send back the answer "We see them with our eyes, we touch them with parts of our bodies, we hear the noises they make, . . ." That might be just what those beings want to know. Perhaps for them it's all a matter of sonar, or something we do not even understand. But even if that answer is just what the aliens want, is it what we want in philosophy?

I think we recognize that the philosophical question is not simply a request for information of this kind. What we want, rather, is some kind of *explanation* of our knowledge—some account of how it is possible. But what kind of explanation of its possibility? Our friends in space could send back a message pressing us for details. "Exactly how does seeing work?", they might ask. "What has to happen after light strikes your eye in order for you to know something about what is reflecting the light? How can you recognize the objects around you and pick them out from the background? Please send detailed explanation." We could send answers to some of their questions. We might even send them as much as we can of our science as it is and let them figure it out for themselves. Maybe they would send back better explanations than we've now got. That would be super naturalized epistemology, if not supernaturalized epistemology.

But would it be what we seek in philosophy? Sending them that information would be like sending them what we know about motion and acceleration, from which they could easily deduce that Achilles will have no trouble overtaking the Tortoise. Would that meet Zeno's challenge? What puzzles us in that case, if anything does, is how it is possible for Achilles to overtake the Tortoise *if* what Zeno relies on at each step of the argument is true. We want to know how overtaking is possible given those undeniable facts invoked by Zeno. That is how that challenge is to be met—not simply by reminding us of the obvious facts of motion and acceleration, or, worse still, by running off and overtaking a tortoise oneself.

The same is true in the case of our knowledge of the world. It is not enough simply to know something; and not just any explanation of how such knowledge is possible will do. It is true that we come to know of the things around us by seeing and touching them, but that is just the sort of information we could send to the aliens in space. Only they or others similarly removed from us would seek that kind of answer. The philosophical question has not yet been reached.

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We want a general answer to the question. It should be expressed in terms of a general "way of knowing." And we find that general source in what we call "the senses" or "sense-perception." The problem then is to explain how we can get any knowledge at all of the world around us on the basis of sense-perception. But again, not just any explanations will do, any more than just any relevant information about motion and acceleration will answer Zeno's question. When our friends in space request such explanations we do not understand them to be asking a philosophical question about our knowledge. What *we* want is an explanation of how we could get any knowledge of things around us on the basis of sense-perception, given certain apparently undeniable facts about sense-perception.

The difficulty comes in philosophy when we try to see exactly how sense-perception works to give us knowledge of the world. We are led to think of seeing, or perceiving generally, in a certain way. What is in question is our knowledge of anything at all about the world, of any of the truths that are about things around us. The difficulty in understanding how sense-perception gives us knowledge of any such truths is that it seems at least possible to perceive what we do without thereby knowing something about the things around us. There have been many versions of that fundamental idea. But whether it is expressed in terms of "ideas" or "experiences" or "sense data" or "appearances" or "takings" or "sensory stimulations," or whatever it might be, the basic idea could be put by saying our knowledge of the world is "underdetermined" by whatever it is that we get through that source of knowledge known as "the senses" or "experience." Given the events or experiences or whatever they might be that serve as the sensory "basis" of our knowledge, it does not follow that something we believe about the world around us is true. The problem is then to explain how we nevertheless know that what we do believe about the world around us is in fact true. Given the apparent "obstacle," how is our knowledge possible?

It is an "obstacle" because it seems to make our knowledge impossible, just as the facts cited by Zeno seem to make overtaking impossible. If several different possibilities are all compatible with our perceiving what we do, the question is how we know that one of those possibilities involving the truth of our beliefs about the world does obtain and the others do not. That would seem to require an inference of some sort, some reasonable hypothesis or some form of reasoning that could take us from what we get in sense-perception to some proposition about the world around us. That hypothesis or principle of inference itself either will imply some-

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thing about the world around us or it will not. If it does, it belongs among those propositions our knowledge of which has yet to be explained, so it cannot help explain that knowledge. If it does not, how can our acceptance of it lead to knowledge of the way things are around us? If it itself implies nothing about such things, and we could perceive what we do without knowing anything about such things, how is our knowledge to be explained? If we are in fact in that position, how is our knowledge of the world around us possible?

The problem is too familiar to need further elaboration here. I have wanted to stress only how very special a question it is about the possibility of knowledge, and what one must do to bring it before our minds in its proper philosophical form. That alone is thought to be enough to show that the question is frivolous or idle. The alleged "obstacle" to our knowledge is thought to be easily avoidable. Even if that quite special question cannot be answered satisfactorily, there is felt to be no good reason to ask it in the first place. The "assumptions" on which it is based are held to be wrong, misguided, and in any case not inevitable.

One familiar criticism is that the whole project is based on the mistaken assumption that there are or must be sensory "foundations" of our knowledge of the world which are in some way "epistemically prior" to the knowledge they serve to support. Abandon that assumption, it is suggested, and the whole problem, or the need to answer it as formulated, disappears. "Enlightened" epistemologists have accordingly moved beyond that quaint doctrine known as "foundationalism." They seek a "nonfoundational" theory of knowledge.

There is not time to go carefully into that complicated issue here. I think the suggestion does not penetrate very deeply into the sources of skepticism; it seems to me to get things almost exactly upside down. And regarding it as simply a matter of deciding to adopt or not to adopt a certain "assumption" is just another way of not taking skepticism seriously. But if we ignore or reject out of hand the familiar traditional question I have tried to identify, what is left?

Suppose we abandon, or never reach, the idea or hope that our knowledge of the world around us is to be explained as being derived from some knowledge or experience that is not itself knowledge of the world around us—something that is "prior to" or "underdetermines" the knowledge we are interested in. What would we then need a philosophical "theory of knowledge" for? It might seem that we would simply have liberated ourselves from an unrealistic restriction, and we could then go ahead and simply explain how our knowledge is possible. But if we are free to explain

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it in terms of sense-perception that *does* amount to knowledge of the things around us, can we ever properly understand *all* our knowledge of the world—how any of it is possible at all?

The "liberated" question can easily be answered by saying that we know of the things around us by perceiving them. We see them, we touch them, we hear them, and so on. We even read about them in the newspaper. But that was just the sort of information we could send to the aliens in space. Is that the sort of thing we want to find out about our knowledge of the world when we wonder, as we do in philosophy, how any of it is possible? Obviously not. We already know all that. If it were the job of a "nonfoundational theory of knowledge" to give us answers like that, it would be even more tedious than skeptical "foundational" theories are now widely held to be.

I do not say that such "enlightened" "theories" or explanations could never tell us anything we do not already know. Obviously, when they got down to the physiological details, they could. But I think there is something we aspire to in the philosophical theory of knowledge that such explanations would not give us. We want an account of our knowledge of the world that would make all of it intelligible to us all at once. We want to see how knowledge of the world could come to be out of something that is not knowledge of the world. Without that, we will not have the kind of doubly general explanation we seek. I think skepticism in epistemology now represents, and perhaps always did represent, the possibility that such an explanation is impossible; that we cannot consider all our knowledge of the world all at once and still see it as knowledge. Given that project, the threat is that skepticism will be the only answer. That alone would not straightforwardly imply that we can know nothing of the world around us—that we can never know whether there is a bus drivers' strike in Rome, for example. But it would suggest that a certain kind of understanding of our position in the world might be beyond us. Taking that possibility seriously, trying to see whether it is so, and if so why, would then be what taking skepticism seriously would amount to. To dismiss it simply on the grounds that we do know many things and that it would be ridiculous to believe we do not would be like assuring us that Achilles will overtake the Tortoise, and that it would be ridiculous to believe that he will not. And we will be in a position to dismiss it on the ground that it is absurd even to seek the kind of understanding philosophers have sought of our knowledge only when we understand better what that goal is, why we seek it, why it is unattainable, and what a philosophical "theory of knowledge" that did not aspire to it would look like.

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TAKING SKEPTICISM SERIOUSLY*

TO put the matter much too simply, Richard Rorty (and others) have held that the skeptical obsession (and perhaps the skeptical crisis) in modern epistemology is the product of a foundationalist demand that knowledge be grounded in indubitable contents of consciousness. Professor Wilson challenges this as a historical thesis. In the writings of early modern philosophers, Descartes for example, skepticism was not the result of failed foundationalist demands, but, she tells us, played quite a different role. Its assigned task was instrumental: it was used to clear the field of competitors so that the new science could prosper and a new foundation of knowledge could be laid on uncluttered ground. Professor Stroud's thesis is not primarily historical, but he too denies that skepticism is essentially or even primarily grounded in the demand for and subsequent failure of foundationalism. Skepticism, he thinks, remains even when foundationalism is abandoned. The upshot of this is that we have been given good reasons for taking skepticism more seriously, both in writing history of philosophy and in doing philosophy itself.

Now although I agree with most of what Wilson and Stroud say, it is worth noting that neither of them takes skepticism seriously as a philosophical position. For both, skepticism is instrumental, though in different ways. For Wilson, historically, the function of skeptical arguments is therapeutic: they serve to purge the mind of common-sense prejudice and outdated theory. For Stroud, skeptical arguments present us with puzzles, albeit deep puzzles. Our philosophy is deepened—we are awakened from our dogmatic slumbers—by addressing ourselves to these arguments. But neither Wilson nor Stroud thinks of skepticism as an independent, self-standing, philosophical position. Stroud reflects on the possibility that the challenge of skepticism may prove impossible to meet, which, for him, is equivalent to saying that philosophy itself is impossible. Here's another alternative: philosophy is possible, and, thought through, skepticism is the form it takes. In the time available, I shall elaborate on this unlikely thesis.

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*Abstract of a paper to be presented in an APA symposium on Skepticism, December 28, 1984, commenting on Margaret D. Wilson, "Skepticism without Indubitability," and Barry Stroud, "Skepticism and the Possibility of Knowledge," this JOURNAL, this issue, 537–544 and 545–551, respectively.

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I COULD NOT HAVE DONE OTHERWISE—SO WHAT?*

WHEREVER progress is stalled on a philosophical problem, a tactic worth trying is to find some shared (and hence largely unexamined) assumption and deny it. The problem of free will is such a problem, and, as Peter van Inwagen notes:

> . . . almost all philosophers agree that a necessary condition for holding an agent responsible for an act is believing that the agent *could have* refrained from performing that act.[1]

Perhaps van Inwagen is right; perhaps most philosophers agree on this. If so, this shared assumption, which I will call CDO (for "could have done otherwise"), is a good candidate for denial, especially since there turns out to be so little to be said in support of it, once it is called in question. I will argue that, just like those people who are famous only for being famous, this assumption owes its traditional high regard to nothing more than its traditional high regard. It is almost never questioned. And the tradition itself, I will claim, is initially motivated by little more than inattentive extrapolation from familiar cases.

To engage the issue, I assert that it simply does not matter at all to moral responsibility whether the agent in question could have done otherwise in the circumstances. Now how does a friend of CDO set about showing that I am obviously wrong? Not by reminding me, unnecessarily, of the broad consensus in philosophy in support of the CDO principle, or by repeating it, firmly and knowingly. The inertia of a tradition is by itself scant recommendation, and if it is claimed that the assumption is not questioned because it is obvious or self-evident, I can at least ask for some supporting illustration of the self-evidence of the assumption in application to familiar cases. Can anyone give me an example of someone withholding a judgment of responsibility until he has determined (to his own satisfaction) whether the agent could have done otherwise?

It will perhaps appear that I must be extraordinarily inattentive to the topics of daily conversation if I can ask that question with a straight face. A prominent feature of many actual inquiries into the

*To be presented in an APA symposium on Freedom and Determinism, December 30, 1984. Peter van Inwagen will comment; see this JOURNAL, this issue, 565–567.

[1] "The Incompatibility of Free Will and Determinism," *Philosophical Studies*, XXVII, 3 (March 1975): 185–99, p. 188, reprinted in Gary Watson, ed., *Free Will* (New York: Oxford, 1982): 46–58, p. 50.

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responsibility of particular agents is the asking of the question "Could he have done otherwise?" The question is raised in trials, both civil and criminal, and much more frequently in the retrospective discussions between individuals concerning blame or excuse for particular regretted acts of omission or commission.

Before turning to a closer examination of those cases, it is worth noting that the question plays almost no role in discussions of praise or reward for felicitous, unregretted acts—except in the formulaic gracious demurrer of the one singled out for gratitude or praise: "What else could I do?" ("Anyone else would have done the same." "Shucks; 'twarn't nothin'.") And in these instances we do not take the agent to be disavowing responsibility at all, but just declaring that being responsible under those conditions was not difficult.

Perhaps one reason we do not ask "Could he have done otherwise?" when trying to assess responsibility for good deeds and triumphs is that (thanks to our generosity of spirit) we give agents the benefit of the doubt when they have done well by us, rather than delving too scrupulously into facts of ultimate authorship. Such a charitable impulse may play a role, but there are better reasons, as we shall see. And we certainly do ask the question when an act is up for censure. But when we do, we never use the familiar question to inaugurate the sort of investigation that would actually shed light on the traditional philosophical issue the question has been presumed to raise. Instead we proceed to look around for evidence of what I call a pocket of *local fatalism*: a particular circumstance in the relevant portion of the past which ensured that the agent would not have done otherwise (during the stretch of local fatalism) *no matter what he had tried, or wanted, to do.* A standard example of local fatalism is being locked in a room.[2]

If the agent was locked in a room (or in some other way had his will rendered impotent), then independently of the truth or falsity of determinism and no matter what sort of causation reigns within the agent's brain (or Cartesian soul, for that matter), we agree that "he could not have done otherwise." The readily determinable empirical fact that an agent was a victim of local fatalism terminates the inquiry into causation. (It does not always settle the issue of responsibility, however, as Harry Frankfurt shows[3]; under special

[2] The misuse of this standard example—e.g., in the extrapolation to the theme that, if determinism is true, the whole world's a prison—is described in my *Elbow Room: The Varieties of Free Will Worth Wanting* (Cambridge, Mass.: Bradford/MIT Press, 1984), from which portions of the present argument are drawn.

[3] "Alternate Possibilities and Moral Responsibility," this JOURNAL, LXV, 23 (Dec. 4, 1969): 829–833.

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circumstances an agent may still be held responsible.) And if our investigation fails to uncover any evidence of such local fatalism, this also terminates the inquiry. We consider the matter settled: the agent was responsible after all; "he could have done otherwise." Proving a negative existential is not generally regarded as a problem here. We can typically count on the agent himself to draw our attention to any evidence of local fatalism he is aware of (for if he can show that "his hands were tied" this will tend to exculpate him), so his failures to come forward with any such evidence is taken to be a reliable (but not foolproof) sign that the case is closed.

The first point I wish to make is that if the friends of CDO look to everyday practice for evidence for the contention that *ordinary people* "agree that a necessary condition for holding an agent responsible for an act is believing that the agent *could have* refrained from performing that act," they in fact will find no such support. When the act in question is up for praise, people manifestly ignore the question and would seem bizarre if they didn't. And when assessing an act for blame, although people do indeed ask "Could he have done otherwise?", they show no interest in pursuing that question beyond the point where they have satisfied their curiosity about the existence or absence of local fatalism—a phenomenon that is entirely neutral between determinism or indeterminism. For instance, people never withhold judgment about responsibility until after they have consulted physicists (or metaphysicians or neuroscientists) for their opinions about the ultimate status—deterministic or indeterministic—of the neural or mental events that governed the agent's behavior. And so far as I know, no defense attorney has ever gone into court to mount a defense based on an effort to establish, by expert testimony, that the accused was determined to make the decision that led to the dreadful act, and hence could not have done otherwise, and hence ("obviously") is not to be held responsible for it.

So the CDO principle is not something "everybody knows" even if most philosophers agree on it. The principle requires supporting argument. My second point is that any such supporting argument must challenge an abundance of utterly familiar evidence suggesting that often, when we seem to be interested in the question of whether the agent could have done otherwise, it is because we wish to draw the opposite conclusion about responsibility from that which the philosophical tradition endorses.

"Here I stand," Luther said. "I can do no other." Luther claimed that he could do no other, that his conscience made it *impossible* for him to recant. He might, of course, have been wrong, or have been deliberately overstating the truth, but even if he was—perhaps

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especially if he was—his declaration is testimony to the fact that we simply do not exempt someone from blame or praise for an act because we think he could do no other. Whatever Luther was doing, he was not trying to duck responsibility.

There are cases where the claim "I can do no other" is an avowal of frailty: suppose what I ought to do is get on the plane and fly to safety, but I stand rooted on the ground and confess I can do no other—because of my irrational and debilitating fear of flying. In such a case I can do no other, I claim, because my rational control faculty is impaired. This is indeed an excusing condition. But in other cases, like Luther's, when I say I cannot do otherwise I mean that I cannot because I see so clearly what the situation is and because my rational control faculty is *not* impaired. It is too obvious what to do; reason dictates it; I would have to be mad to do otherwise, and, since I happen not to be mad, I cannot do otherwise.

I hope it is true—and think it very likely is true—that it would be impossible to induce me to torture an innocent person by offering me a thousand dollars. "Ah"—comes the objection—"but what if some evil space pirates were holding the whole world ransom, and promised not to destroy the world if only you would torture an innocent person? Would that be something you would find impossible to do?" Probably not, but so what? That is a vastly different case. If what one is interested in is whether *under the specified circumstances* I could have done otherwise, then the other case mentioned is utterly irrelevant. I claimed it would not be possible to induce me to torture someone *for a thousand dollars*. Those who hold the CDO principle dear are always insisting that we should look at whether one could have done otherwise in *exactly* the same circumstances. I claim something stronger; I claim that I could not do otherwise even in any roughly similar case. I would *never* agree to torture an innocent person for a thousand dollars. It would make no difference, I claim, what tone of voice the briber used, or whether I was tired and hungry, or whether the proposed victim was well illuminated or partially concealed in shadow. I am, I hope, immune to all such offers.

Now why would anyone's intuitions suggest that, if I am right, then if and when I ever have occasion to refuse such an offer, my refusal would not count as a responsible act? Perhaps this is what some people think: they think that if I were right when I claimed I could not do otherwise in such cases, I would be some sort of zombie, "programmed" always to refuse thousand-dollar bribes. A genuinely free agent, they think, must be more volatile somehow. If I am to be able to listen to reason, if I am to be flexible in the right way, they think, I mustn't be too dogmatic. Even in the most pre-

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posterous cases, then, I must be able to see that "there are two sides to every question." I must be able to pause, and weigh up the pros and cons of this suggested bit of lucrative torture. But the only way I could be constituted so that I can always "see both sides"—no matter how preposterous one side is—is by being constituted so that *in any particular case* "I could have done otherwise."

That would be fallacious reasoning. Seeing both sides of the question does not require that one not be overwhelmingly persuaded, in the end, by one side. The flexibility we want a responsible agent to have is the flexibility to recognize the one-in-a-zillion case in which, thanks to that thousand dollars, not otherwise obtainable, the world can be saved (or whatever). But the general capacity to respond flexibly in such cases does not at all require that one could have done otherwise in the particular case, or in any particular case, but only that under some variations in the circumstances—the variations that matter—one would do otherwise. Philosophers have often noted, uneasily, that the difficult moral problem cases, the decisions that "might go either way", are not the only, or even the most frequent, sorts of decisions for which we hold people responsible. They have seldom taken the hint to heart, however, and asked whether the CDO principle was simply wrong.

If our responsibility really did hinge, as this major philosophical tradition insists, on the question of whether we ever could do otherwise than we in fact do *in exactly those circumstances,* we would be faced with a most peculiar problem of ignorance: it would be unlikely in the extreme, given what now seems to be the case in physics, that anyone would ever know whether anyone has ever been responsible. For today's orthodoxy is that indeterminism reigns at the subatomic level of quantum mechanics; so, in the absence of any general and accepted argument for universal determinism, it is possible for all we know that our decisions and actions truly are the magnified, macroscopic effects of quantum-level indeterminacies occurring in our brains. But it is also possible for all we know that, even though indeterminism reigns in our brains at the subatomic quantum-mechanical level, our macroscopic decisions and acts are all themselves determined; the quantum effects could just as well be self-canceling, not amplified (as if by organic Geiger counters in the neurons). And it is extremely unlikely, given the complexity of the brain at even the molecular level (a complexity for which the word 'astronomical' is a vast understatement), that we could ever develop good evidence that any particular act was such a large-scale effect of a critical subatomic indeterminacy. So if someone's responsibility for an act did hinge on whether, at the moment of decision, that decision was (already) determined by a

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prior state of the world, then barring a triumphant return of universal determinism in microphysics (which would rule out all responsibility on this view), the odds are very heavy that we will never have *any* reason to believe of any particular act that it was or was not responsible. The critical difference would be utterly inscrutable from every macroscopic vantage point and practically inscrutable from the most sophisticated microphysical vantage point imaginable.

We have already seen that ordinary people, when interested in assigning responsibility, do not in fact pursue inquiries into whether their fellows could have done otherwise. Now we see a reason why they would be unwise to try: the sheer impossibility of conducting any meaningful investigation into the question—except in cases where macroscopic local fatalism is discovered. What then can people think they are doing when they ask the CDO question in particular cases? Are they really asking the philosophers' metaphysical question about whether the agent was determined to do what he did, but just giving up as soon as the investigation gets difficult and the prospects get dim of striking a lucky negative answer (with the discovery of some local fatalism)? No, for there is a better question they can have been asking all along, a question that stops *for principled reasons* with the conclusion that local fatalism is ruled out (or in). It is better for two reasons: it is usually empirically answerable, and its answer matters. For not only is the traditional metaphysical question unanswerable; its answer, even if you knew it, would be useless.

What good would it do to know, about a particular agent, that on some occasion (or on every occasion) he could have done otherwise than he did? Or that he could not have done otherwise than he did? Let us take the latter case first. Suppose you knew (because God told you, presumably) that when Jones pulled the trigger and murdered his wife at time t, he could *not* have done otherwise. That is, given Jones's microstate at t and the complete microstate of Jones's environment (including the gravitational effects of distant stars, etc.) at t, no other Jones-trajectory was possible than the trajectory he took. If Jones were ever put back into exactly that state again, in exactly that circumstance, he would pull the trigger again—and if he were put in that state a million times, he would pull the trigger a million times.

Now if you learned this, would you have learned anything important about Jones? Would you have learned anything about his character, for instance, or his likely behavior on merely similar occasions? No. Although people are physical objects which, like

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atoms or ball bearings or bridges, obey the laws of physics, they are not only more complicated than anything else we know in the universe; they are also designed to be so sensitive to the passing show that they never can be in the same microstate twice. One doesn't even have to descend to the atomic level to establish this. People learn, and remember, and get bored, and shift their attention, and change their interests so incessantly, that it is as good as infinitely unlikely that any person is ever in the same (gross) *psychological* or *cognitive* state on two occasions. And this would be true even if we engineered the surrounding environment to be "utterly the same" on different occasions—if only because the second time around the agent would no doubt think something that went unthought the first time, like "Oh my, this all seems so utterly familiar; now what did I do last time?"

Learning (from God, again) that a particular agent was *not* thus determined to act would be learning something equally idle, from the point of view of character assessment or planning for the future. A genuinely undetermined agent is no more flexible, versatile, sensitive to nuances, or reformable than a deterministic near-duplicate would be.[4]

So if anyone at all is interested in the question of whether one could have done otherwise in *exactly* the same circumstances (and internal state) this will have to be a particularly pure metaphysical curiosity—that is to say, a curiosity so pure as to be utterly lacking in any ulterior motive, since the answer could not conceivably make any noticeable difference to the way the world went.

If it is unlikely that it matters whether a person could have done otherwise—when we look microscopically closely at the causation involved—what is the other question that we are (and should be) interested in when we ask "But could he have done otherwise?"? Consider a similar question that might arise about a robot, destined (by hypothesis) to live its entire life as a deterministic machine on a deterministic planet. Even though this robot is, by hypothesis, completely deterministic, it can be controlled by "heuristic" programs that invoke "random" selection—of strategies, policies, weights, or whatever—at various points. All it needs is a pseudo-random number generator, either a preselected list or table of pseudo-random numbers to consult deterministically when the occasion demands or an algorithm that generates a pseudo-random sequence of digits. Either way it can have a sort of bingo-parlor machine for providing it with a patternless and arbitrary series of digits on which to pivot some of its activities.

[4] This is shown in "Designing the Perfect Deliberator," in *Elbow Room*, *op. cit.*

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Whatever this robot does, it could not have done otherwise, if we mean that in the strict and metaphysical sense of those words that philosophers have concentrated on. Suppose then that one fine Martian day it makes a regrettable mistake: it concocts and executes some scheme that destroys something valuable—another robot, perhaps. I am not supposing, for the moment, that *it* can regret anything, but just that its designers, back on Earth, regret what it has done and find themselves wondering a wonder that might naturally be expressed: *Could it have done otherwise?* Let us suppose that they first satisfy themselves that no obvious local fatalism (locked room, dead battery) has afflicted their robot. But still they press their question: Could it have done otherwise? They know it is a deterministic system, of course; so they know better than to ask the metaphysical question. Their question concerns the design of the robot; for in the wake of this regrettable event they may wish to redesign it slightly, to make this *sort* of event less likely in the future.[5] What they want to know, of course, is what information the robot was relying on, what reasoning or planning it did, and whether it did "enough" of the right sort of reasoning or planning.

Of course in one sense of 'enough' they know the robot did not do enough of the right sort of reasoning; if it had, it would have done the right thing. But it may be that the robot's design in this case could not really be improved. For it may be that it was making optimal use of optimally designed heuristic procedures—but this time, unluckily, the heuristic chances it took didn't pay off. Put the robot in a *similar* situation in the future, and, thanks to no more than the fact that its pseudo-random number generator is in a different state, it will do something different; in fact it will usually do the right thing. It is tempting to add: it *could* have done the right thing on this occasion—meaning by this that it was well enough designed, at the time, to have done the right thing (its "character" is not impugned); its failure depended on nothing but the fact that something *undesigned* (and unanticipatable) happened to intervene in the process in a way that made an unfortunate difference.

A heuristic program is not guaranteed to yield the "right" or sought-after result. Some heuristic programs are better than others; when one fails, it may be possible to diagnose the failure as assignable to some characteristic weakness in its design, but even the best are not foolproof, and when they fail, as they sometimes must,

[5] "We are scarcely ever interested in the performance of a communication-engineering machine for a single input. To function adequately it must give a satisfactory performance for a whole class of inputs, and this means a statistically satisfactory performance for the class of inputs which it is statistically expected to receive." Norbert Wiener, *Cybernetics* (Cambridge, Mass: Technology Press; New York: Wiley, 1948), p. 55.

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there may be no *reason* at all for the failure; as Cole Porter would say, it was just one of those things.

Such failures are not the only cases of failures that will "count" for the designers as cases where the system "could have done otherwise." If they discover that the robot's failure, on this occasion, was due to a "freak" bit of dust that somehow drifted into a place where it could disrupt the system, they may decide that this was such an unlikely event that there is no call to redesign the system to guard against its recurrence.[6] They will note that, in the microparticular case (as always) their robot could not have done otherwise; moreover, if (by remotest possibility) it ever found itself in exactly the same circumstances again, it would fail again. But the designers will realize that they have no rational interest in doing anything to improve the design of the robot. It failed on the occasion, but its design is nevertheless above reproach. There is a difference between being optimally designed and being infallible.

Consider yet another sort of case. The robot has a ray gun that it fires with 99.9% accuracy. That is to say, sometimes, over long distances, it fails to hit the target it was aiming at. Whenever it misses, the engineers want to know something about the miss: was it due to some *systematic* error in the controls, some foible or flaw that will keep coming up, or was it just one of those things—one of those "acts of God" in which, in spite of an irreproachable execution of an optimally designed aiming routine, the thing just narrowly missed? There will always be such cases; the goal is to keep them to a minimum—consistent with cost-effectiveness, of course. Beyond a certain point it isn't worth caring about errors. W. V. Quine notes that engineers have a concept of more than passing philosophical interest: the concept of "don't-cares"—the cases one is rational to ignore.[7] When they are satisfied that a particular miss was a don't-care, they may shrug and say: "Well, it could have been a hit."

What concerns the engineers when they encounter misperformance in their robot is whether the misperformance is a telling one: does it reveal something about a pattern of systematic weakness, likely to recur, or an inappropriate and inauspicious linking between sorts of circumstances and sorts of reactions? Is this *sort* of thing apt to happen again, or was it due to the coincidental convergence of fundamentally independent factors, highly unlikely to

[6] Strictly speaking, the recurrence of an event *of this general type*; there is no need to guard against the recurrence of the particular event (that is logically impossible) or against the recurrence of an event of *exactly* the same type (that is nomologically impossible).

[7] *Word and Object* (Cambridge, Mass.: MIT Press, 1960), pp. 182, 259.

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recur? So long as their robot is *not* misperforming but rather making the "right" decisions, the point in asking whether it could have done otherwise is to satisfy themselves that the felicitous behavior was not a fluke or mere coincidence but rather the outcome of good design. They hope that their robot, like Luther, will be imperturbable in its mission.

To get evidence about this they ignore the micro-details, which will never be the same again in any case, and just average over them, analyzing the robot into a finite array of *macro*scopically defined states, organized in such a way that there are links between the various degrees of freedom of the system. The question they can then ask is this: Are the links the right links for the task?

This rationale for ignoring micro-determinism (wherever it may "in principle" exist) and squinting just enough to blur such fine distinctions into probabilistically related states and regions that can be *treated as* homogeneous is clear, secure, and unproblematic in science, particularly in engineering and biology. That does not mean, of course, that this is also just the right way to think of people, when we are wondering whether they have acted responsibly. But there is a lot to be said for it.

Why do we ask "Could he have done otherwise?"? We ask it because something has happened that we wish to interpret. An act has been performed, and we wish to understand how the act came about, why it came about, and what meaning we should attach to it. That is, we want to know what conclusions to draw from it about the future. Does it tell us anything about the agent's character, for instance? Does it suggest a criticism of the agent that might, if presented properly, lead the agent to improve his ways in some regard? Can we learn from this incident that this is or is not an agent who can be trusted to behave *similarly* on *similar* occasions in the future? If one held his character constant, but changed the circumstances in minor—even major—ways, would he almost always do the same lamentable sort of thing? Was what we observed a fluke, or was it a manifestation of a robust trend—a trend that persists, or is constant, over an interestingly wide variety of conditions?[8]

When the agent in question is oneself, this rationale is even more plainly visible. Suppose I find I have done something dreadful. *Who cares* whether, in exactly the circumstances and state of mind I found myself, I could have done something else? I didn't do some-

[8] We are interested in trends and flukes in both directions (praiseworthy and regretted); if we had evidence that Luther was just kidding himself, that his apparently staunch stand was a sort of comic-opera coincidence, our sense of his moral strength would be severely diminished. "He's not so stalwart," we might say. "He could well have done otherwise."

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thing else, and it's too late to undo what I did. But when I go to interpret what I did, what do I learn about myself? Ought I to practice the sort of maneuver I botched, in hopes of making it more reliable, less vulnerable to perturbation, or would that be wasted effort? Would it be a good thing, so far as I can tell, for me to try to adjust my habits of thought in such sorts of cases in the future? Knowing that I will always be somewhat at the mercy of the considerations that merely happen to occur to me as time rushes on, knowing that I cannot entirely control this process of deliberation, I may take steps to bias the likelihood of certain sorts of considerations routinely "coming to mind" in certain critical situations. For instance, I might try to cultivate the habit of counting to 10 in my mind before saying anything at all about Ronald Reagan, having learned that the deliberation time thus gained pays off handsomely in cutting down regrettable outbursts of intemperate commentary. Or I might decide that, no matter how engrossed in conversation I am, I must learn to ask myself how many glasses of wine I have had every time I see someone hovering hospitably near my glass with a bottle. This time I made a fool of myself; if the situation had been quite different, I certainly would have done otherwise; if the situation had been virtually the same, I might have done otherwise and I might not. The main thing is to see to it that I will jolly well do otherwise in (merely) similar situations in the future.

That, certainly, is the healthy attitude to take toward the regrettable parts of one's recent past. It is the self-applied version of the engineers' attitude toward the persisting weaknesses in the design of the robot. Of course, if I would rather find excuses than improve myself, I may dwell on the fact that I don't *have* to "take" responsibility for my action, since I can always imagine a more fine-grained standpoint from which my predicament looms larger than I do. (If you make yourself really small, you can externalize virtually everything.) But we wisely discourage this refuge in finer-grained visions of our embedding in the world, for much the same reason it is shunned by the engineers: what we might learn from such an investigation is never of any consequence. It simply does not matter whether one could have done otherwise.

It does not matter for the robot, someone may retort, because a robot could not *deserve* punishment or blame for its moments of malfeasance. For us it matters because we are candidates for blame and punishment, not mere redesign. You can't *blame* someone for something he did, if he could not have done otherwise. This, however, is just a reassertion of the CDO principle, not a new consideration, and I am denying that principle from the outset. Why indeed shouldn't you blame someone for doing something he could not

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have refrained from doing? After all, if he did it, what difference does it make that he was determined to do it?

"The difference is that if he was determined to do it, then he *had no chance not to do it.*" But this is simply a *non sequitur,* unless one espouses an extremely superstitious view of what a chance is. Compare the following two lotteries for fairness. In Lottery A, after all the tickets are sold, their stubs are placed in a suitable mixer, and, after suitable mixing (involving some genuinely—quantum-mechanically—random mixing if you like), the winning ticket is blindly drawn. In Lottery B, this mixing and drawing takes place *before* the tickets are sold, but otherwise the lotteries are conducted the same. Many people think the second lottery is unfair. It is unfair, they think, because the winning ticket is determined before people even buy their tickets; one of those tickets is *already* the winner; the other tickets are so much worthless paper, and selling them to unsuspecting people is a sort of fraud. But in fact, of course, the two lotteries are equally fair: *everyone has a chance of winning.* The timing of the selection of the winner is an utterly inessential feature. The reason the drawing in a lottery is typically postponed until after the sale of the tickets is to provide the public with first-hand eyewitness evidence that there have been no shenanigans. No sneaky agent with inside knowledge has manipulated the distribution of the tickets, because the knowledge of the winning ticket did not (and could not) exist in any agent until after the tickets were sold.

It is interesting that not all lotteries follow this practice. Publishers' Clearinghouse and Reader's Digest mail out millions of envelopes each year that say in bold letters on them "YOU MAY ALREADY HAVE WON"—a million dollars or some other prize. Surely these expensive campaigns are based on market research that shows that in general people do think lotteries with pre-selected winners are fair so long as they are honestly conducted. But perhaps people go along with these lotteries uncomplainingly because they get their tickets for free. Would many people *buy* a ticket in a lottery in which the winning stub, sealed in a special envelope, was known to be deposited in a bank vault from the outset? I suspect that most ordinary people would be untroubled by such an arrangement, and would consider themselves to have a real opportunity to win. I suspect, that is, that most ordinary people are less superstitious than those philosophers (going back to Democritus and Lucretius) who have convinced themselves that, without a continual supply of genuinely random *cruces* to break up the fabric of causation, there cannot be any real opportunities or chances.

If our world is determined, then we have pseudo-random number

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generators in us, not Geiger counter randomizers. That is to say, if our world is determined, all our lottery tickets were drawn at once, eons ago, put in an envelope for us, and doled out as we needed them through life. "But that isn't fair!" some say, "For some people will have been dealt more winners than others." Indeed, on any particular deal, some people have more high cards than others, but one should remember that the luck averages out. "But if all the drawings take place before we are born, some people are *destined* to get more luck than others!" But that will be true even if the drawings are held not before we are born, but periodically, on demand, throughout our lives.

Once again, it makes no difference—this time to fairness and, hence, to the question of desert—whether an agent's decision has been determined for eons (via a fateful lottery ticket lodged in his brain's decision-box, waiting to be used), or was indeterministically fixed by something like a quantum effect at, or just before, the moment of ultimate decision.

It is open to friends of the CDO principle to attempt to provide other grounds for allegiance to the principle, but since at this time I see nothing supporting that allegiance but the habit of allegiance itself, I am constrained to conclude that the principle should be dismissed as nothing better than a long-lived philosophical illusion. I may be wrong to conclude this, of course, but under the circumstances I cannot do otherwise.

DANIEL C. DENNETT

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DENNETT ON 'COULD HAVE DONE OTHERWISE'*

DANIEL DENNETT attacks what he describes as a "shared assumption" of writers on free will:

> *A* is responsible for having done *X* only if *A* could have refrained from doing *X*.

If this sentence is to express a thesis that has been widely accepted, then 'could have' must be read as the past *indicative* of 'can', where

*Abstract of a paper to be presented in an APA symposium on Freedom and Determinism, December 30, 1984, commenting on Daniel C. Dennett, "I Could Not Have Done Otherwise—So What?," this JOURNAL, this issue, 553–565.

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'can' means 'to be able to'. That is, this sentence must be interpreted this way:

> *Was Able* *A* is responsible for having done *X* only if *A* was able to refrain from doing *X*.

But Dennett's arguments show that he is really attacking *this* principle:

> *A* is responsible for having done *X* only if *A* could have refrained from doing *X* in exactly the same circumstances.

Here 'could have' is a past *subjunctive* form. The words following 'only if' are a subjunctive conditional, a "'might' conditional":

> *Might Have* *A* is responsible for having done *X* only if (*A* was in exactly the circumstances *A* was actually in $\Diamond\!\!\rightarrow$ *A* refrained from doing *X*).

Since only an incompatibilist would accept *Might Have*, Dennett is not attacking a "shared assumption" of all writers on free will. Moreover, *Might Have* can hardly be described as an *assumption*, even of those incompatibilists who accept it. They accept *Might Have* because it is a consequence of *Was Able* and incompatibilism: *Might Have* is a "foreseen but unintended consequence" of their position.

So Dennett's dogs are barking up the wrong tree. What would happen if they were directed to the right one? Could his arguments and examples be used to cast doubt on *Was Able*? (Or perhaps I should say, cast further doubt. Though *Was Able* is indeed a widely shared assumption in the free-will debate, Harry Frankfurt has probably shown that it is false.[1]) I do not see how to turn Dennett's arguments against *Was Able*; for his arguments are *essentially* directed against a principle containing the qualification 'in exactly the same circumstances'. And this qualification makes sense only when it is applied to a subjunctive 'could have' (='might have'); it makes no sense to apply it to an indicative 'could have' (='was able to').

Now if Frankfurt is right and *Was Able* is false, then those incompatibilists who accept *Might Have* because *Might Have* is a consequence of *Was Able* and incompatibilism, accept *Might Have* on the basis of a false premise—at *least* one. But even if Frankfurt is right, there are *true* propositions that, in conjunction with incompatibilism, entail *Might Have*.[2] Dennett's arguments, therefore,

[1] "Alternate Possibilities and Moral Responsibility," this JOURNAL, LXV, 23 (Dec. 4, 1969): 829–833.

[2] See my paper "Ability and Responsibility," *Philosophical Review*, LXXVII, 2 (April 1978): 201–224, or my book *An Essay on Free Will* (Oxford: Clarendon Press; New York: Oxford, 1983), pp. 162–182.

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if they refute *Might Have*, refute incompatibilism by *Modus Tollens*. This fact shows us the most profitable way to read Dennett's paper: as an attack on incompatibilism. Dennett is not really denying an assumption made by all the parties to the free-will debate; he is joining one of the parties.

But, looked at this way, Dennett's arguments are not really new. Don't they come down to this? To say that *A* could have done *X* is to ascribe to him the following feature: he would have done *X* if he had tried (or wanted) to—he was not "trapped in a pocket of local fatalism"—, and, in relevantly similar future circumstances, he may well try (or want) to.

PETER VAN INWAGEN

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CHANCE, REALISM, QUANTUM MECHANICS*

IS quantum mechanics incompatible with realism? Although many philosophers and physicists would answer Yes to this question, solid arguments for the incompatibility are hard to come by: too often, it is not realism, but some parochial version of realism (e.g., *Every object has a definite spatial location*) whose incompatibility with QM is argued for; too often also it is not QM that is argued to be nonrealistic but only some particular, usually problematic, interpretation of it. In this paper I present an argument for incompatibility which comes at the matter from an unusual direction and which involves only standard, textbook versions of realism and of QM. With respect to QM, there are of course some very strange ideas in the textbooks, on which no one would want his argument to rest—most conspicuously, the usual

*To be presented in an APA symposium of the same title, December 30, 1984. Allen Stairs will comment; see this JOURNAL, this issue, 578/9.

In order to be printed here, the original version of this paper had to be drastically cut. Among the topics I most regretted deleting were these: why some of the artificialities involved in seeing all talk about chance as talk about *C* might disappear, given a better, relativistically acceptable story about measurement; why one cannot treat QM as a realistic theory by counting among its axioms statements about what the actual frequencies will be (such a theory would treat as impossible what QM leaves open as possible, and so is not QM); why axioms about what the frequencies would be, in an infinite sequence of trials, won't help either (in addition to being again incompatible with QM, such axioms are one and all false—if it is possible that this coin be flipped infinitely often and never come up heads, then to say that if it were flipped the limiting frequency of heads would be .5 is to say what is false); a comparison with Isaac Levi's views in *The Enterprise of Knowledge* (Cambridge, Mass.; MIT Press, 1980) (not so different from my own, I think, as must first appear); chance outside of QM (I don't think there is any).

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story about measurement and the Projection Postulate. When it is helpful to have a particular interpretation of QM in mind, I shall speak as if this familiar story were correct; nonetheless, I hope it will be clear how little my argument depends on the truth of this story; some form of my argument can be given, I think, for any version of QM that speaks of causally undetermined events whose probabilities are calculated from the wave functions.

Just now I used the ambiguous word 'probabilities' rather than the much more precise word 'chances'. This was deliberate; although there is these days something approaching a consensus among philosophers that the probabilities that QM allows us to calculate are in fact chances, i.e., propensities or objective tendencies, I can see no such consensus among the writers of physics texts: *their* use of the word 'probability' is neither so clear nor so clearly disentangled both from frequency views and from subjective (probability$_1$) elements as is the philosophers' use of the word 'chance'. Now it is one of the main theses of this paper that the physicists have done well not to embrace wholeheartedly the reading of probabilities as chances; indeed, most of this paper, beginning in the next section, consists of a skeptical attack on the notion of chance in QM. The connection with realism, though crucial to the argument, emerges only at the end.

I

Let us suppose for the sake of argument that QM tells us about chances, and let us begin by asking what chances are—or, to put the question in "semantical" form, when we use the word 'chance' what are we talking about? As a first step toward answering this semantical question, I propose we get clear about the syntax of statements about chance. Imagine QM formalized as part of a formalization of our language (in first-order logic, perhaps with a modal operator L: "it is a law of nature that"). I will suppose that the quantifiers range over the following, at least: particles, sets of particles, states, observables, and times. How will we formalize our talk about chance? I suggest that, given sufficient expressive power in the rest of the language, everything we want to say about chance can be said in terms of either of two relations: $C(S, A, a, b, x)$, to be read as "any quantum system in state S has a chance x of displaying a value for the observable A in the interval of reals $[a,b]$, upon being measured for A"; and $C^*(M, t, A, a, b, x)$, to be read as "the quantum system M has at t the chance x of displaying an A value in $[a,b]$, on A measurement." Of these two relations, C^* has more the look of "single-case propensities"; C on the other hand treats chances as attributes of kinds of trials—both approaches to chance are familiar in the literature. In the case of QM it seems likely that

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all single-case propensities can be "read off" the state of the system being measured, so that C^* is definable from C. Using the operator L, C is also definable from C^*; it seems to me to be a matter of taste which of these one uses.

As for the adequacy of C and C^* in allowing us to say what we want to say about chances, one will want to test this by looking at cases. These fall into three classes. First, statements like "This electron has chance .5 of displaying 'spin-up'" and its technically more difficult continuous-spectrum variants, e.g., "This electron has a .5 chance of being found in region R." These have straightforward expressions in terms of either C or C^*. Second, statements about repeated measurements, e.g., "There is a high chance that roughly half of the silver atoms will be deflected upward." Sometimes statements of this kind can be regarded as falling into the first class; e.g., if the silver atoms were all measured at once, they could be regarded as forming a single quantum system on which a single observable is being measured. But if the silver atoms are measured one after the other, than the formal treatments I am familiar with do not allow us to count them as a single system on which one observable is being measured. To handle such cases, I suggest that we take advantage of the fact that, for such noninteracting systems, QM always treats the individual chances as independent, and translate what we say about the chances of various over-all frequencies into statements about the products of the individual chances. Finally, we sometimes talk about conditional chances: "The chance that electron 2 will turn out spin-up, if electron 1 does, is 0." Here, I suggest we make use of the usual definition of conditional in terms of unconditional chances, and take ourselves to be speaking of the ratio between the chance that the measurement of spin on the two electrons will give the respective values 1/2 and 0 (this is of course measurement of a single observable) and the chance that measurement of spin on the first electron gives the value 1/2.

What sorts of objects are C and C^*? They are *relations-in-intension*: that is, they have the form of relations (in Frege's phrase, they are *unsaturated*), but they should not without argument be identified with their extensions—they may interact with counterfactuals in ways in which their extensions do not. In fact, in the case of C, such identification seems harmless: one does not contemplate the possibility, in QM, that the chances associated with the QM states might have been different from what they in fact are. In the case of C^*, however, it seems undeniable that certain counterfactual claims can be made about C^* which would not hold of the corresponding extension, e.g., that this electron might, under other circumstances,

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have had at t a different chance of spin-up. Extensions, being sets, cannot have been other than what they are; so in this sense, C^* is an intentional entity. In another sense, however, neither C^* nor C is intensional: use of their names does not produce intentional contexts; one can substitute expressions with the same denotations in the scope of 'C' or 'C^*', salva veritate. The point is worth emphasizing. One hears so often of analogies between chance and the modalities—the intentionality of probability locutions, the apparent similarity of the questions "Is it possible?" "Is it probable?"—that one begins to suppose that a semantical analysis of chance statements must be something like a possible-world semantics. Partly because the states and observables take over the roles of possible worlds, partly because the basic syntax turns out very simple, there turns out to be no need to treat chance in QM as a modality; chance—whether we take it to be C or C^*—is a relation-in-intension. The reason this matters is that if we want to know whether some sort of reductionist thesis can be maintained in the case of chance, then we now know how to discuss the issue: most of what we know about reduction has to do specifically with the case of relations-in-intension.

Consider now the relation-in-intension K, which I define by writing out the standard "definition" of C that one would give in QM. That is, I take as given the notion of inner product of pairs of QM states (when these are states of compatible type, e.g., both possible states of a single electron or both possible states of a pair of electrons), and I treat observables as functions from states to states (not always defined); then, in the special case where observable A is a projection operator, we have $K(S, A, 1, 1, x)$ iff the inner product $(S, A(S)) = x$. (This is the easiest case to write out; the full definition can be found in the standard texts.) Let $K^*(M, t, A, a, b, x)$ similarly be the QM "definition" of C^*. Then the question I want to ask is, Does $C = K$? That is, do the expressions 'C' and 'K' denote the same physical relation? The usual discussions of such questions concern one-place relations, i.e., properties—and usually properties of concrete objects; for this reason we might also want to ask whether $C^* = K^*$ and also whether, for example, the property of having a chance at t of .5 of yielding 1 on A-measurement is the same property as $K^*(\ \ , t, A, 1, 1, .5)$.

In all these cases, the fact that the properties in question are coextensive by physical law should strongly incline us to suppose that they are identical. We should be careful, however: there are apparent exceptions to the principle that distinct properties are never lawfully coextensive. What I would claim, however, is that none of the considerations that make the exceptions plausible has

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any analogue here. In particular, nearly all the exceptions are driven by our intuition that two properties of a whole which supervene on different properties of different parts may count as distinct, even if lawfully coextensive; indeed, one may feel that possession of such a supervenient property can have causal efficacy only where and when the relevant parts are located. (I say "when" in order to include the cases where the whole is an enduring object and the parts on which a property of the whole supervenes are temporal segments.) Nothing of this sort applies in the present case—one cannot think, for example, of the present propensities of the electron as in some way supervening on its future behavior. Since the only other source of counterexamples to our general principle of property identity—Kripke's direct-access arguments—has likewise no bearing on this case, I think we have good reason to conclude that $C = K$. To put the matter a little crudely, to speak of chances in QM is to speak about wave-function amplitudes.

II

If $C = K$—if in talking about chances we are talking only about the wave function and its overlap with other wave functions—then why bother having two names for one thing? Why not dispense entirely with talking about chances and speak only of wave-function amplitudes? My answer to this question will, finally, be that we can and should do exactly that. But this answer will meet with resistance: surely, the notion of chance would not have occasioned as much discussion as it has if people had thought it was dispensable! As a first step toward overcoming this resistance, I want now to look at the role that chance plays in QM or in our thinking about QM; the question I will always be asking is: Can K play this role equally well?

What do we need the notion of chance for, in QM? One thought that sometimes seems to be lurking in the background when people talk of the "propensity interpretation" of QM is that chances are needed to interpret the QM states—i.e. to tell us what the wave functions really are. This idea seems to me to presuppose either that the QM states are really only ways of talking about the classical states or that all theoretical terms must be tied individually to observables; I shall dismiss it out of hand. The QM *states* do not need to be individually interpreted or tied piecemeal to observations; it is rather the entire *theory* QM that must have the right sorts of connections—explanatory and predictive—with the phenomena. And here indeed chances seem to play a crucial role. Let us see how this works.

Consider a typical case in which we use QM to explain our observations. A beam of silver atoms is sent through a Stern-Gerlach

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apparatus, and the density of silver on each of the targets is measured: the density on the upper target is twice what it is on the lower. The explanation is that the amplitudes for spin-up, spin-down, respectively (along the appropriate axis), were, say, $\sqrt{2/3}$, $i\sqrt{1/3}$, respectively, and that the experiment constitutes a measurement for spin.

One is inclined to feel that this explanation is incomplete, that what has been said about the amplitudes has not yet been shown to have anything to do with the measured frequencies. The missing factor, one thinks, is mention of chance: if we say that, according to QM, the squared amplitudes give the chances for the appropriate events to occur, then the explanation is complete.

How is that mentioning the connection between amplitudes and chances has allowed us to complete the explanation? According to the causal model of explanation which many people accept today, when an explanation is felt to be incomplete, what has been left out is a piece of the causal story. This can happen in either of two ways (or some combination thereof): the explanation may have neglected to mention an entity, event, or property crucial to the causal process by which the explanandum came about; or the explanation may have omitted mention of some of the lawlike connections that would show us how the actors in the causal drama did their work. In the present case, we will have to be very liberal indeed about the notion of a causal process if we are to apply either of these accounts: for the sake of argument let us suppose that there is some kind of legitimacy in speaking of an outcome that was not determined by initial conditions as being produced by a quasi-causal process. Even making this assumption, it seems to me difficult to apply either account to the case of QM. Consider the first. I have already argued that to introduce chance into the story is not to introduce a new actor into the causal drama; chance = K is mathematically definable from the state vector. Even if one were not then persuaded by my argument, one should now ask: How could it possibly help if we *did* introduce chance into the explanation, as a property distinct from K and K^*, and with a causal efficacy all its own?—or How could it help to suppose that the event of *atom a having a chance of* 1/2 *of spin-up* is distinct from the event of *atom a having a squared amplitude of* 1/2 *of the spin-up component* and that the former event has a quasi-causal efficacy that the latter lacks? However one imagines the chances as "causally" operative—as "bringing about the frequencies" or whatever—there is no reason not to attribute these "effects" simply to the wave function. Whatever role chance plays in the explanation, it cannot be as a new actor in the causal drama.

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Turning now to the second account, the most plausible way to apply it to the case of QM is to say the following: although '*C*' and '*K*' denote the same relation, they have different senses. The information that we mentally file under the heading 'chance' or '*C*' is distinct from the information we file under the heading '*K*' or 'state vector'; this is information that is crucial to showing how the explanandum comes about, but it can be brought into the explanation only by explicitly saying that *K* is *C*, that the wave-function amplitudes in fact give chances. The difficulty with this account is as follows: if it were correct, we could fill out the explanation without ever saying anything about chance: we could simply make explicit as information about *K* all that we know about *K* under its alias 'chance'. But there is no such information! No doubt those few remaining philosophers—and many physicists—who subscribe to some version of the long-run-frequency theory or of its obscure cousin the ensemble theory, would be content to say that what we know about *K* qua chance is this: in a large ensemble, or in a long run of experiments, the proportion of silver atoms in the upper wing will be 2/3. But most of us nowadays realize that no nontrivial statements about what the frequencies are or will be are entailed by statements about chances; statements about chances entail only other statements about chances. And there is no other way to make explicit without circularity the information we are supposed to store under the heading 'chance'; talk of tendencies, propensities, aptitudes for bringing about the frequencies, are only picturesque ways of speaking once more about chances.

What role does chance play in the explanation? The answer is quite easy once we abandon the idea that chances help us to fill out a causal or quasi-causal story. If I know of one of our silver atoms that the chance of its entering the upper wing is 2/3, the I will assign a subjective probability 2/3 to the proposition that it will enter the upper wing. If I know only the wave amplitudes and have not yet been told that the wave amplitudes give the chances, then my credal state is not so constrained. The contribution of chances is, I suggest, to fix our subjective probabilities. This is a contribution whose importance goes well beyond the case we've been discussing: the subjective probabilities of QM play a large variety of roles: in testing the theory or new hypotheses within the theory, in using the theory in practical decision making, and in much else besides. In the case under discussion, to speak of the wave amplitudes as "giving chances" does not make the explanation more adequate as an account of how the explanandum came about; instead, it makes the explanation more adequate in a sense more closely related to the (nowadays somewhat unpopular) idea that a good ex-

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planation, if offered in time, would have allowed us to predict the explanandum. When we measure observables on ensembles of particles, the QM chances instruct us to assign subjective probability close to 1 to the belief that the measured frequency will fall in a certain narrow range; in the vast majority of cases, this belief turns out correct. I do not think it matters much that this is not strictly prediction, nor does it matter whether we say that this sort of quasi-predictive feature makes our QM explanations better *as explanations*; it is enough to say that, having started out with a sense that something important was added to our explanation by bringing in the notion of chance, we now have a plausible characterization of what that something is: chances bring us to the subjective probabilities. The connection between the two is the principle of direct inference that (roughly) subjective probabilities should follow our knowledge of the chances—what David Lewis calls the "Principal Principle."[1]

The connection between chances and subjective probabilities has been pointed out by many writers, many of whom, I suspect, would regard the Principal Principle as saying everything that needs to be said about what chance is and what role it plays. It seems to me, however, that our original question about chance has in no way yet been answered; rather, it appears now in a sharper form: if the role of chance is to mediate the transition from wave-function amplitudes to subjective probabilities, then why is such mediation needed? Why do we not simply assign subjective probabilities on the basis of what we know about the wave functions and never talk about chances at all? Is the reason only that people are used to letting their subjective probabilities follow the known chances—that the Principal Principle plays the role of a "meaning postulate" for the word 'chance', but that we have no such meaning postulate governing the use of the words '*K*' or 'wave function'? I think there are deeper reasons than this. For one, I believe that many people implicitly subscribe to the idea that to derive our subjective probabilities from chances is to show these probabilities to be justified in a sense in which they could never be justified if we derived them directly from the wave-function amplitudes. Such a view would of course make little sense if by 'justified' one understood something like "derived by accepted inferential procedures"; it rather presupposes some notion of "objective" justification: that there are some facts about the world in virtue of which it is rational for us—in our best interest—to adopt the subjective probabilities we do adopt,

[1] See his "*A Subjectivist's Guide to Objective Chance*," in W. L. Harper, R. Stalnaker, and G. Pearce, eds., *Ifs* (Boston: Reidel, 1980), pp. 267–297.

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and that speaking about chances somehow alludes to these facts, entails them, or explains them.

There is more than one version of this idea that one could put forward; I suspect that it is clear by this time that, and why, I would consider *any* version of it to be mistaken. For what could be the "facts about the world" that "objectively justify" our choice of subjective probabilities? Once we have given up on the idea that to talk of chance is to allude to some events or states additional to the ordinary QM events and states, the only serious candidates remaining are the facts about the actual frequencies; and indeed these do provide a kind of "objective justification" for our subjective probabilities: a bookie who posts odds on Stern-Gerlach experiments in accordance with the usual subjective probabilities will break even. But these facts about the actual frequencies are no less available to us if we entirely omit speaking of chances and base our subjective probabilities directly on K values, than they are if we use chances to mediate the connection: in either case, we will assign a high subjective probability to the belief that the frequencies will turn out in such a way as to "objectively justify" our subjective probabilities. Nor does speaking of chances help us to explain why the actual frequencies will turn out this way (supposing that they will), not if 'explain' means "causally account for"; if to explain is to quasi-predict in the sense we discussed earlier, than we can explain the frequencies whether or not we talk about chances.

III

I have to this point suggested two reasons why we feel compelled to speak of chances in QM: habit—we don't feel comfortable basing direct inferences immediately on wave amplitudes—and error—we imagine that the notion of chance can do a kind of quasi-causal explanatory work which it cannot possibly do. I want now to suggest a third reason, which I believe underlies the other two. Consider the following argument:

> The reason we cannot dispense with talking about chances is simply this: if we do not allow ourselves the notion of chance, we will find it impossible to say what the theory QM *is*. To see this, just imagine writing out the theory. For much of what the theory has to tell us, this will in principle present no problems: QM makes a large number of statements about the states of systems: which states are allowed in various situations, how they evolve, what values observables take in various states, etc.; one can of course write these down. The problem arises when we try to write down the part of the theory that has to do with probabilities. It is not debatable that QM has *something* to do with probabilities: someone who accepted everything that QM had to tell us about allowed states and their evolution, but assigned inap-

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propriate subjective probabilities to statements about outcomes of measurements, could not be reasonably said to have accepted QM: he would not be able to give the same explanations, make the same predictions, or draw the same conclusions from tests that the rest of us do. But if to accept QM is to assign certain subjective probabilities, this can only be because of something QM *says*: there must be some statements QM makes about the world such that to accept these statements as true is to be logically compelled to assign the appropriate subjective probabilities. And what could such statements be, if not statements about chances? Not statements about frequencies: except when it tells us that certain outcomes are impossible or necessary, QM never tells us anything categorically about what the frequencies will be—though of course to accept QM is *inter alia* to assign subjective probabilities to statements about frequencies. And not statements about wave-function amplitudes: one can accept such statements without being logically forced to assign the QM subjective probabilities. No other candidates being even remotely plausible, I conclude that QM must make statements about chances.

I believe that this argument is importantly right about one thing: to accept QM is to do more than merely accept-as-true all the statements QM makes about the world. Where the argument goes wrong is in seeing this as a problem; in any case, the inadequacy of the "solution" is apparent: if acceptance of statements about the wave functions doesn't rationally compel correct subjective-probability assignments, it won't help matters to invent a new name, 'chance', for the wave amplitudes and then claim that, as a matter of "logic," acceptance of statements about chance *does* compel correct subjective-probability assignments. If the inferential move from statements about the wave function to assignments of subjective probability is not justified, what have we been told about chances that makes the inferential move from chances to probability assignments look any better? The simple solution to the difficulty is to realize that there is no inferential move from wave amplitudes to probabilities, neither directly nor by way of chances. Rather, the maxim "Let your probability assignments follow the squared amplitudes!" is an independent part of the theory, not entailed by *any* statements the theory makes. And that means that the aspect of QM which has to do with probabilities is not expressed by any statements at all.

What is a scientific theory? To answer this question one should first ask, what is it to accept a theory? And an answer to this question—vague, but correct as far as it goes—is this: to accept a theory is to determine one's cognitive state. What this answer makes apparent is that our conception of what a theory is will depend greatly on what we take a cognitive state to be. Until recently

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nearly all writers on philosophy of science or philosophy of mind implicitly took our cognitive state as consisting of something like a list of sentences, the set of one's beliefs. To accept a theory was to add to, and perhaps also subtract from, the list. Given such a conception of cognitive state, and of what it is to accept a theory, the following view of the nature of theories was almost inevitable: a theory is a list of sentences; to accept a theory is to add it to one's list of beliefs, deleting whatever beliefs contradict the theory. Lately this orthodoxy has increasingly been called into question, as a growing number of writers have started taking seriously various richer notions of cognitive state. The new accounts (rather, newly popular accounts—there is little here that would have seemed strange to de Finetti or Carnap) add to a (perhaps empty) corpus of accepted statements—the counterpart of the old list—a set of statements believed with varying degrees of subjective probability. Some such model of our cognitive state has been taken for granted throughout this paper. What I want to point out now is that such richer conceptions of our cognitive state allow for a richer conception of what it is to accept a theory: if to accept a theory is to determine our cognitive state, such determination need not consist only in altering the corpus; accepting the theory may also involve altering the subjective probabilities. And if these alterations cannot themselves be seen as arising by a uniform process from the ways in which we alter the corpus, then we will be led to the following conception of a theory: a theory consists in a list of sentences, together with a list of instructions for altering one's subjective probabilities; in accepting the theory, one adds the list to one's corpus, and one obeys the instructions.

The abstract possibility just contemplated is, I believe, realized in the case of quantum mechanics: if I am right, QM is a theory which, in addition to giving us a great deal of information about the world, also instructs us about how to fix subjective probabilities. This is the reason that QM is a nonrealist theory: although what it says about the world is either true or false, a list of instructions cannot be true or false; if QM* differs from QM only in assigning slightly different probabilities and if I accept QM and you accept QM*, then there is no proposition I assert that you deny, no sense in which my beliefs are a more accuarate picture of the world than yours. As I see it, the notion of chance is a purely formal device which we use to give QM the look of a realistic theory: if we use this device, we can say that QM consists only of a list of statements to add to the corpus; some of these are statements about chances, and these "entail" that we assign the appropriate probabilities. But these "entailments" are nothing like real entail-

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ments—no semantics shows the "conclusion" true if the premise is; there is not even an argument to show that it is in our interest to draw the conclusion, given the premise; to the extent that the device relies on the pretense that we have here a kind of entailment, it can only mislead us. There is another way in which the device is unfortunate: it depends on our refusing to answer the question: Is $C = K$? i.e., Are chances just squared wave amplitudes? For if the answer is Yes, then chance cannot do the work we want it to do, and if the answer is No, then we have introduced into our physical account a new property that has no causal role to play. Scientific—and philosophical—methodology does not allow us to leave such questions unanswered; this, I suspect, is why controversy over the nature of chance has persisted for so long. All of this seems a heavy price to pay in order to maintain the illusion of realism; we would do better to ask ourselves why it is that we find this illusion so difficult to give up.

STEPHEN LEEDS

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PROBABILITY AND REALISM IN QUANTUM MECHANICS*

STEPHEN Leeds begins his paper by noting that "most of us" suspect that there is something to the idea that quantum mechanics presents a nonrealistic view of the world. He also notes that the arguments typically given for this claim ought not to convince us, but he undertakes to provide us with an argument that should. I agree with Leeds's assessment of the usual arguments, but, unlike the majority to whom he alludes, I do not share the sense that quantum mechanics is a nonrealist theory. Furthermore, Leeds's arguments do not move me any closer to such a view. My comments will be devoted to explaining why I find these arguments unconvincing.

Briefly, Leeds focuses his attention on probability in quantum mechanics and asks whether there is any interesting role for a concept of chance in interpreting the theory. He argues that chance (or, in any case, his relation C) is identical with a relation K, defined purely by reference to the overlap between vectors in Hilbert

*Abstract of a paper to be presented in an APA symposium on Chance, Realism, and Quantum Mechanics, December 30, 1984, commenting on a paper by Stephen Leeds; see this JOURNAL, this issue, 567–578.

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space. Leeds rejects the idea that the concept of chance is needed to *interpret* the wave function, claiming that this idea rests on an outmoded view of what is involved in interpreting a theory. He also rejects the suggestion that an appeal to chance could function in any sort of causal explanation of quantum phenomena; in particular, since statements about chance can yield only other statements about chance, we cannot explain the observed distributions by invoking chances. According to Leeds, the only other role that the concept of chance could play is to "mediate the transition from wave-function amplitudes to subjective probabilities." But if this is so, we can dispense with talk of chance altogether and simply talk about wave functions and their amplitudes. Further, even if we accept various statements about the wave function, this can never rationally compel us to assign subjective probabilities in any particular way. This all leads to the fundamental nonrealist conclusion: the part of quantum mechanics that has to do with probabilities is not expressed by *statements*, but is essentially a set of *instructions* that we accept when we accept the theory. Thus, quantum mechanics does not *tell* us anything about probabilities. At best, it instructs us to assign probabilities in a certain way. And since instructions are neither true nor false, this part of quantum mechanics cannot be interpreted realistically.

I think this view can be accepted only if one drastically simplifies both the problems involved in the interpretation of quantum mechanics and our understanding of the theory itself. The trouble begins with Leeds's discussion of the relationship between C and K. I will argue that, whether or not we ultimately decide to invoke chance as an interpretive tool, to identify K with C would be to confuse the representation with the thing represented. In some contexts this sort of identification is harmless, but this is not one of those contexts. If we move beyond this sort of identification, we will see that the question of "interpreting the wave function" is actually much richer than Leeds allows and may have a solution that renders his view unacceptable.

In the same vein, the problem of understanding probability in quantum mechanics is many-layered, and Leeds's presentation of it hides a great deal of important complexity. I will try to bring out aspects of the issue that Leeds ignores and will use this discussion to sketch a reading of quantum mechanics which allows for something much closer to a realist understanding of probability in quantum mechanics than Leeds believes possible.

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FORMAL JUSTICE, MORAL COMMITMENT, AND JUDICIAL PRECEDENT*

DESPITE the encroachment of legislation on matters that used to fall within the "common law," the judicial practice of following decisions made in prior cases remains important and is likely to continue so. For decisions must be rendered where legislation has not yet intervened, appears to conflict, requires problematic interpretation, or has troubling applications.

When past decisions were justly made or created justifiable rules, following them is morally unproblematic (unless, of course, a new case presents special difficulties). When past decisions cannot be justified on their merits, justification for respecting precedent may be sought in the justice of the legal system as a whole. But these conditions, I assume, are not always satisfied, and so the reasons they give for following precedent are likewise limited. Other reasons are often given. It is often said, for example, that following judicial precedent avoids frustrating expectations, enhances security, and minimizes risks entailed in useful ventures. These reasons are also limited in application.

No such limitation applies to the idea, sometimes suggested,[1] that respect for precedent is required by the principle that like cases should be treated alike. I shall call this *the formal justice argument*. One does not find the argument developed whole within the literature, and there are, theoretically at least, indefinitely many ways of developing it. But one might sketch the reasoning as follows. The requirement that like cases be treated alike is widely regarded as fundamental to justice. It is taken to imply that, once we have dealt with a situation, either in judgment or in action, it is incumbent on us to deal with similar situations in a similar way, unless we find adequate reason to deal with them differently. From this it may seem a simple step to respect for precedent, because that seems just a special case of following our past practice generally. That is the type of view I wish to challenge.

I. THE FORMAL JUSTICE ARGUMENT

The premise of the argument is *the principle of justice*:

> Justice requires that like cases be treated alike and that different cases be treated differently.

*To be presented in an APA symposium on Philosophy of Law, December 30, 1984. Jeffrie G. Murphy will be cosymposiast; see this JOURNAL, this issue, pp. 587–593. Thomas Scanlon will comment; his paper is not available at this time.

[1] Cf., e.g., Martin Golding, *Legal Reasoning* (New York: Knopf, 1984), p. 98, and Neil MacCormick, *Legal Reasoning and Legal Theory* (New York: Oxford, 1978), pp. 73ff.

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which is sometimes regarded as expressing the very concept of justice. This ignores *non*comparative aspects of justice. For the purpose of this paper, however, I shall assume that justice at least includes a comparative aspect, expressed by the principle of justice.

The conclusion of the argument is *the principle of precedent,* which I formulate so as to emphasize that I consider it here only as a requirement of justice:

> Justice requires that judicial decisions follow prior decisions in similar cases.

This formulation ignores the judicial practice of "distinguishing" cases that are relevantly different, which corresponds to the "different cases" clause of the principle of justice. The issues appear analogous, however; so for the sake of economy I will ignore the problem of "distinguishing." But we must understand the principle of precedent to hold only "other things being equal."

Because the principle of justice is not limited in application, the formal justice argument implies that there is *always* a sound reason of justice for following precedent, so that *any* departure requires justification. It implies, most importantly, that precedents have this moral force *regardless* of their disutility, their injustice, and the injustice of the legal system as a whole.

The principle of justice requires that like cases be treated alike, etc., but it does not tell us which cases are alike or different. Until it is supplemented by "criteria of relevant similarities and differences," the requirement, it is said, "cannot afford any determinate guide to conduct."[2] But the principle of precedent, by contrast, seems to provide some guidance for decision (though its guidance will be limited by relevant differences between cases and conflicting precedents). It does so, of course, only if we can identify "similar" cases. But that problem seems to have a clear solution. The *judicial* practice of precedent involves deciding new cases by reference to facts about them that are the same as facts about prior cases that were considered by the courts that decided them as grounds for certain legal consequences. The principle of precedent requires (other

[2] H. L. A. Hart, *The Concept of Law* (New York: Oxford, 1961), p. 155. Hart endorses reasoning parallel to the formal justice argument, *ibid.*, p. 156; I discuss it in "On Formal Justice," *Cornell Law Review*, LVIII, 5 (June 1973): 833–861. Hart disavows the implications of the formal justice argument in *Essays on Bentham* (New York: Oxford, 1982), pp. 147–153, where he criticizes Ronald Dworkin's apparent acceptance of its implications; cf. Dworkin, *Taking Rights Seriously* (Cambridge, Mass.: Harvard, 1978), pp. 326/7. Dworkin disavows the point in Marshall Cohen, ed., *Ronald Dworkin and Contemporary Jurisprudence* (Totowa, N.J.: Rowman & Allanheld, 1984), pp. 256–260. The present paper explains what is at issue in that exchange.

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things being equal) that the same facts in new cases be taken as grounds for the same legal consequences.

One might infer from this contrast that the principle of precedent could not possibly be derived from the principle of justice without significant supplementary assumptions. For, it might be said, one cannot extract a "substantive," action-guiding principle from a substantively "empty," purely "formal" principle. However plausible that line of reasoning may be, we should not apply it here. For it would beg an important question.

The principle of justice can be read as assuming that certain similarities and differences between "cases" are relevant from the standpoint of justice and that others are irrelevant. It is compatible with the view that certain "criteria of relevant similarities and differences" are in principle *discoverable*, though they may vary with the context. If that were so, it would be a matter of objective fact that certain cases are alike and others different for the purpose of doing justice, and the principle of justice would be no less "substantive" or action-guiding than other such principles, which do not explicitly *tell* us how to determine when the conditions they lay down are in fact satisfied. We are not entitled to assume the contrary.

Those who believe that the principle of justice requires mere *consistency* in judgment or action, so that *any* departure from past ways of dealing with cases requires justification, must either reject the reading of the principle that I have just described or else must hold that past practice *constitutes* the *creation* of relevant similarities and differences. These come down to the same idea, namely, that relevant similarities and differences are (at least to some degree) *subjective* rather than objective. This can be seen in the context of the formal justice argument, as follows.

Someone who endorses the principle of precedent is committed to the proposition that certain ways of acting are just and that other ways of acting are unjust. Someone who endorses the formal justice argument is committed to the proposition that this is a matter of treating like cases alike or failing to do so. The upshot is a commitment to the following proposition:

> For the purpose of doing justice, two cases *are relevantly similar and should be decided similarly* (unless there are overriding relevant differences between them) when factual aspects of one to be decided are the same as factual aspects of one that has been decided by a court's having taken those facts as grounds for certain legal consequences.

The formal justice argument assumes, in other words, that the decision of a court of law, that certain legal consequences are justi-

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fied by certain factual aspects of a case, *makes* a subsequent case with corresponding aspects relevantly similar for the purpose of doing justice.

The question is why we should believe this to be so.

II. CONSERVATIVE JUSTICE

The heart of the formal justice argument would seem to be the notion that justice requires consistency in judgment or action, so that any departure from past judgment or practice requires justification in terms of relevant differences. This involves a commitment to the following general claim:

> For the purpose of doing justice, two situations are *relevantly similar and should be dealt with similarly* (unless there are overriding relevant differences between them) when factual aspects of one to be dealt with are the same as factual aspects of a situation that one has dealt with by taking those facts as grounds for moral judgment or for morally appropriate action.

The formal justice argument, in other words, would seem to assume that the principle of justice entails, as a corollary, the following *principle of conservative justice*:

> Justice requires that one deal with (judge or act within) new situations in the same way one has dealt with past similar situations, unless there are overriding relevant differences between them.

There is a tradition of understanding the requirement that like cases be treated alike and different cases differently so that it demands this kind of consistency. But the tradition has not grappled with the question, "What is the moral force of a past decision that was itself mistaken?" That is the point at issue, and it is especially important in the context of judicial decision.

It might be thought that the principle of conservative justice is akin to a conservative principle that requires one to maintain beliefs until one has adequate reason to reject them. The parallel epistemological principle would be something like this:

> Reason requires that one judge (form beliefs about or explanations of) new situations in the same way one has judged past similar situations.

It seems clear, however, that such a principle would not be thought to hold with respect to judgments that one has found adequate reason to reject. I wish to make the parallel claim about the limits of conservative consistency.

One might justifiably come to believe (one might even come to know) that one has dealt wrongly with a type of situation that one now encounters again. This might happen just when one faces the

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same sort of situation for the second time, in the absence of any intervening occasion for re-examination.

Clearly, one must be cautious here. Circumstances may promote self-deception. But these complications should not be exaggerated. Factors tempting one to moral self-indulgence may not be present. And, of course, similar problems affect one's finding that two similar situations are relevantly different. Such contingent practical complications may be put aside when considering what justice itself requires. My point is that there seems no reason to suppose that justice requires conservative consistency in judgment or action, when one is indeed justified in believing that one had previously judged or acted wrongly.

If this is right, one might wonder why the requirement that like cases be treated alike, etc., has been assumed to require conservative consistency. A plausible explanation would have to take several factors into account. First, there are the practical complications already mentioned. If moral self-indulgence is a frequent temptation, more common than the repetition of mistakes, it may be wise counsel to accept a practical presumption in favor of going on as before. This is closely related to the second factor, which is that principles can be understood to have "pragmatic requirements" concerning their justified applications. However conscientious we may be, we cannot always succeed in satisfying the principles we seek to follow. So, for example, one may be justified in judging or acting in a certain way, on the basis of justified beliefs about what the relevant principles require, even when one fails to do what they actually require. When one has dealt with a situation on its merits as one responsibly perceived them, the principle of justice can be understood pragmatically to imply that it is incumbent on one to deal with similar situations similarly. That is easily confused with the notion of conservative justice.

And there may be special reasons for such a presumption in a judicial context, where it can be assumed that someone will be adversely affected by a decision, however one decides. Consistency of judicial decision tends to satisfy expectations, and the like. There may be good and adequate reason to respect or establish a conservative practice of judicial precedent. But wise counsel is one thing, and regarding the principle of conservative justice as a corollary of the requirement that like cases be treated alike is quite another. The difference is a matter of principle and a matter of understanding the grounds that may justify particular institutional arrangements. Although such factors may normally be present in a judicial context, that is not necessarily the case, and they surely cannot be

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assumed in other contexts to which the principle of justice applies, such as individual decision. When the contingent conditions that argue for conservative consistency are not satisfied, we would seem to have no need to justify a departure from past practice. Justice does not argue for the repetition of mistakes.

One special type of case might seem to provide support for the notion of conservative justice. Suppose a case could not be decided on its merits but called for a more or less arbitrary drawing of lines for future reference. Such a decision might have a claim to be respected independent of the utility of doing so. But this would not show that justice requires respect for mistakes, for the original decision as described cannot be regarded as a mistake. Nevertheless, such a precedent may be thought to have some moral force even if the decision was not the wisest or most fortunate choice. But when such a precedent has claims to be respected, that would seem to be because the original decision *constitutes a commitment*, made to others, that future decisions in similar cases shall be made similarly. It is not that the commitment *follows from* a general requirement to go on as before, as conservative justice would have it, but rather that the commitment to go on as before *accounts for* the requirement to do so.

Such a commitment presupposes a special context (though not necessarily a judicial context). Circumstances must make it reasonable for others to regard the decision as such a commitment. This helps to explain why we often speak not merely of expectations but specifically of *legitimate* expectations. I do no wrong to others merely by frustrating their reasonable expectations about my future conduct, but I can wrong them by frustrating expectations *that I have made* "legitimate" by committing myself to behaving in a certain way; for example, by committing myself to deciding future cases in a similar way. I will return to this point in a moment.

My conclusion is that conservative justice requires qualification and that conservative consistency is not a corollary of the principle that like cases should be treated alike.

III. INSTITUTIONAL CONSISTENCY

If one could assume the doctrine of conservative justice, the next step toward the principle of precedent by this route would seem obvious. The principle of conservative justice would seem to entail, as a special case, what may be called a "judge-centered" principle of conservative justice:

> Justice requires that one decide new cases in the same way one has decided similar past cases, unless there are overriding relevant differences between them.

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For, as I suggested earlier, it may seem a simple step from this to the principle of precedent.

But this does not follow. For the principle of precedent requires a judge to follow decisions made by *other* judges, not just her own past decisions. The principle of conservative justice argues for individual consistency, whereas the principle of precedent requires, roughly speaking, *institutional* consistency.

This would be speaking very roughly. The practice of following precedent in the real judicial world is governed by doctrines that determine whether any past decisions are to be regarded as "binding" for courts within a given jurisdiction and, if so, which decisions, for which cases, and to what degree. These doctrines vary from one jurisdiction to another.

But the principle of precedent, which has no such limitations, can still be considered relevant. If the principle of precedent is sound, it provides a reason for respecting those judicial doctrines which authorize the practice of precedent, as well as a reason to establish such a practice, a reason to be guided by precedent when the law does not prohibit judges from doing so, and even a reason to follow precedent when the law does not permit it.

The question remains, however, how one can get from a conservative principle of justice or its corollaries, which require that *one's own* decisions be consistent, to the much more general and demanding principle of precedent, which requires *in addition* that one's decisions be consistent with those of others. I see no way of bridging that gap.

If we cannot derive the principle of precedent from the principle of justice, not even from a principle of conservative justice, we may still be able to account for the moral requirement, if there is one, of institutional consistency, that is, the requirement (other things being equal) that officials render decisions that are maximally consistent with decisions rendered by other officials, at least within the same jurisdiction.[3] We might go some way toward understanding this by regarding it (at least in part) as the implication of a commitment undertaken by officials. Just as officials normally can be understood to make a commitment of fidelity to law, which involves a commitment to following the specific laws of a jurisdiction, so far as that is possible, they can be understood as well to commit themselves to the next best thing, prior official interpretations of the law, when those are needed and relevant.

Some might regard this sort of commitment as an essential aspect of occupying an official position, but I would regard it differ-

[3] Cf. Dworkin, *Taking Rights Seriously*, *op. cit.*, pp. 87ff.

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ently. That is because we are here considering not a merely legal but a moral commitment, which rests on mutual understanding rather than formal oaths or legal requirements. It is therefore limited in the manner of all such commitments. I do not mean merely that it is not "absolute" but only "prima facie," but also that a binding moral commitment has limited conditions of acquisition and of scope. It is doubtful that such a commitment is binding on an office-holder who is coerced to serve. And it is doubtful that it can apply at all within legal systems that are fundamentally corrupt or outrageously unjust.[4]

These reflections take us far from the formal justice argument. I should emphasize that I am not denying that there may be good reason, including reasons of justice, for the practice of judicial precedent. I am inclined to believe, however, that all such reasons are contingent on the satisfaction of conditions that are not always satisfied within a judicial system. By considering the formal justice argument, I hope to have provided some justification for that more general conclusion.

DAVID LYONS

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JUSTIFYING DEPARTURES FROM EQUAL TREATMENT*

THE injunction that we should "treat like cases alike" is often put forth as a fundamental principle of justice and rationality. Procedural justice seems to require such treatment (at least in a great many cases) in order that institutionally encouraged expectations not be thwarted—one possible reason behind the moral power of legal precedents. Retributive justice would seem to require such treatment also; for how can a system of punishment give to each his just deserts if equally deserving persons are treated differently? So too for any other kind of justice where the idea of "equal protection of the laws" has application. Basic to the arguments from justice, however, may be an even

[4] I argue for these points in "Justification and Judicial Responsibility," *California Law Review*, LXXII, 2 (March 1984): 178–199, and in "Derivability, Defensibility, and the Justification of Judicial Decisions," forthcoming in *The Monist*, LXVIII, 2 (April 1985).

*To be presented in an APA symposium on Philosophy of Law, December 30, 1984. David Lyons will be co-symposiast, see this JOURNAL, this issue, pp. 580–587. Thomas M. Scanlon will comment; his paper is not available at this time.

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deeper idea that it is simply *irrational* not to treat like cases alike. Leibniz's Principle of Sufficient Reason (very likely to be a part of any acceptable theory of rationality) may informally be put this way: given two objects or states of affairs *A* and *B*, a rational being will not prefer *A* over *B* unless that being can find some relevant property or attribute possessed by *A* but not by *B* which justifies (is the sufficient reason for) the preference.

Once one gets beyond intuitions and slogans, however, the principle "treat like cases alike" may be seen as plagued with many problems. For example, (1) What makes two cases alike in relevant respects? We need, of course, a theory of relevance; and it may be very difficult to articulate such a theory that does not beg the very questions we hoped to illuminate with the principle.[1] (2) When we realize that our previous decisions involved error (were even substantively unjust), are we bound—even prima facie—to continue our pattern of error?[2] Of course there is now a difference—namely, that we have changed our moral (or legal) view about the cases. But this is not a difference *in* the cases—not a difference that makes *them* unlike. It is more a difference in *us*. (3) In some contexts, is it not sometimes both just (not unjust) and rational (not irrational) to prefer one object or person over a relevantly similar object or person? If it is correct that nobody has a right to be saved by me, that my saving someone is supererogatory on my part, then may I not permissibly save drowning Jones but not save equally drowning Smith simply because I *like* Jones better than Smith?[3]

In spite of these problems, let us suppose for a moment that there is a class of cases where we know (reasonably well) what the principle "treat like cases alike" means and where the principle is properly regarded as morally and legally binding. The question I

[1] One way of interpreting Kant's Categorical Imperative is as an instantiation of the Principle of Sufficient Reason: claim no liberty for yourself that you could not, as a rational being, extend to relevantly similar people in relevantly similar circumstances. This appears to make good on Kant's promise to derive the ultimate principle of morality from the concept of rationality. A problem with this, however, is that the concept of relevant similarity will no doubt have to be explicated in moral terms—a circularity which will prevent the Categorical Imperative from being the ultimate principle of morality.

[2] The classic expression of this worry is that of Jonathan Swift: "It is a maxim among these lawyers that whatever has been done before may legally be done again; and therefore they take special care to record all the decisions formerly made against common justice and the general reason of mankind. These, under the name of precedents, they produce as authorities." For an argument that "justice does not argue for the repetition of mistakes," see David Lyons' symposium paper, *op. cit.*

[3] If one says that my likings can themselves constitute a part of the relevant differences between the two situations, the door is opened to real problems. How can the Principle of Sufficient Reason be a test of the rationality of my preference between *A* and *B* if my preference can itself count as a relevant difference?

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wish briefly to explore is this: in the cases where the principle "treat like cases alike" clearly is prima facie binding, under what circumstances may it legitimately be overridden? Human social and legal practices are not precision instruments, and some of them will inevitably leave the result that like cases are treated differently. When is this outcome tolerable, and when is it intolerable? Given the space limitations set for this paper, I cannot possibly develop an adequately detailed theory on this issue in the present context. I can at least, however, indicate the kinds of philosophical questions that ought to be asked and give some hint of how one might start to answer these questions. Let us limit ourselves to a legal context and raise this question: Under what circumstances might a state action be permissible even if it resulted in like cases receiving different treatment?

The prevailing doctrine in U.S. constitutional law is that such outcomes are tolerable in the vast majority of cases. Consider Justice Potter Stewart's remarks in the 1970 case of *Dandridge v. Williams*:

> In the area of economics and social welfare, a State does not violate the Equal Protection Clause merely because the classification made by its laws are imperfect, if the classification has some "reasonable basis." It does not offend the Constitution simply because the classification "is not made with mathematical nicety or because in practice it results in some inequality." The problems of government are practical ones and may justify, if they do not require, rough accommodations—"illogical, it may be, unscientific." A statutory discrimination will not be set aside if any state of facts reasonably may be conceived to justify it (397 U.S. 471).

The standard here being articulated by Justice Stewart is often called the "rational-basis test" (though it would be better named the "not-irrational-basis test"), and the level of judicial review involved is often called "minimal scrutiny." The standard, applied in the vast majority of cases where equal-protection objections are raised, is this: state action, even if it results in inequality, will pass judicial review if it serves a purpose that could be regarded as rational. This is clearly a very weak test, since it should be possible to think of some plausible reason in defense of all but the most silly state actions.

If the principle "treat like cases alike" is really so basic to both justic and rationality, how can such a weak standard be involved in defense of that principle—a standard that allows frequent departures from it? At least three kinds of consideration might be relevant here. (1) The inequality of treatment might result from a social practice—e.g., wide administrative or judicial discretion in

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certain areas of the law—which is judged to be of greater social value than is strict equality of outcome in these areas. Juvenile sentencing often results in like cases' being treated very differently, but this is often defended by the argument (perhaps ill founded) that the paternalistic function of juvenile courts would not properly be served unless judges in juvenile cases had wide discretion.[4] (2) Requiring agencies to develop standards or procedures that would guarantee the elimination of unequal outcomes might be more costly than it would be worth. This might place a crippling burden on the state. Economic efficiency often requires that some matters (for example, the setting of automobile insurance rates) be handled in actuarial terms (e.g., a function in part of such statistically relevant factors as age and sex); and this makes it certain, e.g., that some persons who are actually very safe drivers will pay a premium as high as careless drivers. Since care in driving seems to be the relevant attribute, the safe-driving young male will no doubt resent as unjust the fact that he pays rates higher than a young female or older male who is no more careful (perhaps less careful) than he is. But if insurance companies were required to individuate in such a way as to avoid this unequal outcome, the cost of automobile insurance for everyone would be absurdly high. A great many state policies are necessarily and properly based upon actuarial thinking of a comparable nature. (3) It may be judged that certain rights are worthy of protection even if their exercise will result in inequality. "Grandfather clauses" might be defended in this way. The fact that I have been allowed to do something for a very long time (e.g., sell my wares on the city streets) may earn me the right to continue doing this even if no other persons will be allowed to engage in the activity.[5]

These arguments (at least the first two) might appear to be mere appeals to utility, but the case is perhaps actually a bit more complex. If it could be shown (as it probably could be, for example, in the insurance case) that even the persons treated unequally will be better off under the unequal practice than they would be under a practice that eliminated the inequality, then these persons are in no obvious sense being exploited for the general welfare. Thus it is by no means clear that they experience—at least ultimately—any injustice. They are winners too, and thus they no doubt would have rationally willed such an unequal practice in a Rawlsian original position.

[4] See Ellen Canacakos, "Indeterminate Juvenile Sentencing: A Denial of Equal Protection?" *Arizona Law Review*, XXII (1980): 141.

[5] *New Orleans v. Duke*, 427 U.S. 297 (1976).

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Let us suppose that it is justified to override the demand for equal treatment in cases such as those above. Why not allow such an override in all cases? Why should courts ever use any standard other than minimal scrutiny and the rational-basis test that such scrutiny involves?

The hornbook answer is this: The rational-basis test is perfectly appropriate when matters of only "economics and social welfare" are involved. Sometimes, however, something of considerably greater importance is at stake—namely, the *fundamental rights* of citizens. (Rights that have been identified by the courts as fundamental include such enumerated rights as freedom of speech, press, and religion and such "penumbral" rights as privacy.) When these rights are encumbered or when special burdens are placed on members of "suspect classifications" (e.g., racial minorities), "strict judicial scrutiny" is triggered. This involves the "compelling state interest/least restrictive alternative" test: state action will pass constitutional review only if the encumbrance of the right is justified by a *compelling* or *overriding* (not merely rational or legitimate) state interest and if the encumbrance is actually necessary to accomplish that interest. This would seem to be a very tough test to pass, a test that in many ways incorporates the liberal/libertarian view on rights, liberty, and state coercion. Expanded beyond equal-protection analysis to do some work in "substantive due process," the test looks as though it could be a very powerful tool indeed to check government intrusions into the basic rights and liberties of citizens.[6]

But is it? One cannot answer this big question, of course, until one gets beyond the edifying slogans and answers two basic questions of philosophical analysis. (1) What does it mean to say and how does one support the claim that some rights are *fundamental* and others are not? (Why do economic rights not currently enjoy such status? Is this just a matter of shifting opinion and ideology? If so, what is interestingly fundamental about rights so grounded?) (2) What does it mean and how does one support the claim that a particular state interest is *compelling* and not merely rational or

[6] I have recently attempted to apply the framework of the compelling-state-interest test in evaluating state aims for the practice of punishment. See my "Retributivism and the State's Interest in Punishment" in J. Roland Pennock and John W. Chapman, eds., *Nomos XXVII: Criminal Justice* (New York: NYU Press, 1984) and my "Retributivism, Moral Education, and the Liberal State," to appear in *Criminal Justice Ethics* in 1985. For a good general discussion of the various levels of judicial scrutiny (including the emerging "intermediate standard of review"), see John E. Nowak *et al.*, *Constitutional Law* (St. Paul: West, 2d ed., 1983), pp. 590 ff.

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legitimate or permissible? Since I have written elsewhere on question 1,[7] I shall confine my remarks here to question 2.

What makes a state interest compelling or overriding? When one considers the interests that have actually been identified as compelling by the Supreme Court,[8] one might wonder whether this is merely a laundry list or whether any coherent moral, political, and legal theory binds all these interests together in a systematic way. There had better be such a theory, of course, or the compelling-state-interest test will hardly count as an important framework in which to structure our thinking about state intrusions into fundamental rights. Mere judicial intuition ("sure seems compelling to me!") will hardly do the job; for the doctrine needs to be a doctrine about what judges *should* value, not about what they do in fact value. Neither can the values or preferences of a democratic majority function to explicate the doctrine—i.e., a compelling state interest cannot be understood simply as a state of affairs deeply desired by a majority of citizens. A strict standard of judicial scrutiny is supposed to protect fundamental rights against majoritarian preference—a job it certainly cannot perform if it is itself interpreted in terms of majoritarian preference. Where then do we go for an analysis of this concept?

One start toward an analysis of the concept of a compelling state interest might be found in some of the traditional devices of liberal

[7] See "Moral Theory and Its Application to Law," ch. 2 of *The Philosophy of Law: An Introduction to Jurisprudence*, by Jeffrie G. Murphy (chs. 1, 2, and 3) and Jules L. Coleman (chs. 4 and 5) (Totowa, N.J.: Rowman & Allanheld, 1984). For an interesting argument in support of fundamental-rights analysis—one that goes beyond American law—see William E. Conklin, *In Defense of Fundamental Rights* (Germantown, Md.: Sijthoff & Noordhoff, 1979). For a powerful attack on fundamental rights analysis, see Paul Brest, "The Fundamental Rights Controversy: The Essential Contradictions of Normative Constitutional Scholarship", *Yale Law Journal*, XC (1981): 1063. For an analysis of the doctrine of "suspect classifications" and an argument that this doctrine "shares a close and complex relationship with notions of fundamental rights," see Lawrence H. Tribe, *American Constitutional Law* (Mineola, N.Y.: Foundation Press, 1978), pp. 1000 ff. See also my "Rationality and Constraints on Democratic Rule," forthcoming in *Nomos*.

[8] Courts are reluctant to apply strict scrutiny, and they often avoid its application by refusing to identify a class as suspect, by denying that the rights encumbered are fundamental, or by denying that the rights, though fundamental, are encumbered to a degree sufficient to justify strict scrutiny. When strict scrutiny is applied, however, the state generally loses—either because it cannot cite a compelling interest or (much more frequently) because it cannot establish that there are no less restrictive means to accomplish its interest. Because there are very few cases where the courts have explicitly upheld state action by agreeing that the interest the state was pursuing was compelling, it is not easy to generate a noncontroversial list of interests identified to date as compelling by the courts. Some interests that seem to have been so identified are the following: national security and public safety (*Korematsu v. United States*, 323 U.S. 214, 1944); maternal health (*Roe v. Wade*, 410 U.S. 113, 1973); and future life as represented in the human fetus (*Roe v. Wade*). The courts also seem to recognize as a compelling interest the protection of those rights which have been identified as fundamental.

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social and political philosophy—the tradition that generated the whole idea of fundamental rights as a foundation for the legal and political order. Consider, for example, the general idea of a social-contract model for rational social and political choice. If a group of persons living in competitive proximity to each other did not have a state or government, what good reasons might they have for *forming* one and accepting the resulting curtailments of liberty that this would entail—i.e., what reasons might they find "compelling" in reluctantly making such a decision? Using Robert Nozick's metaphor, we might also consider the matter in this way: If we think of the state as an agency that we might *hire* (at a cost in both money and liberty) to do a certain job for us, how would we write the job description? What would be worth the price? These questions suggest one possible theoretical answer to the question of how the concept of a compelling state interest might be analyzed: a state interest is to be viewed as compelling if and only if it is one of the interests or goals that one can properly imagine rational persons forming a government to achieve.

This, of course, is only a superficial start toward an analysis of the concept of a compelling state interest, and serious (perhaps fatal) objections to social-contract theory and moral and political liberalism have recently been articulated.[9] In spite of these problems, however, the analysis does have at least one clear virtue: it locates this concept of constitutional law in the context of a general social and political theory that is consistent with the idea of fundamental rights. If this or some other comparable theoretical foundation is not possible, then fundamental-rights analysis and the compelling-state-interest test to which it is tied do not constitute a powerful legal framework at all. They are simply the illusion of one. The philosophy of constitutional law cannot remain isolated from the liberal/anti-liberal debates that currently dominate moral and political philosophy. If liberalism rests on incoherence, then constitutional law (including fundamental-rights analysis and the compelling-state-interest test) probably does so as well, and the task that Ronald Dworkin has set for those who work in the area—to develop a theory of this body of law which will render it coherent and which will be consistent with the best moral and political theories—will be futile.[10]

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[9] One of the most powerful statements of this case is to be found in Alasdair MacIntyre's *After Virtue* (Notre Dame, Ind.: University Press, 1981).

[10] Certain values which seem morally important and which are often cited by the courts as state interests may not—initial appearances to the contrary—be consistent with the liberal political tradition. For an attempt to make such a case with respect to punishment, see my "Retributivism, Moral Education, and the Liberal State," *supra* fn 6.

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HEIDEGGER/DERRIDA—PRESENCE*

TWO authors are named. Neither will be present—though one could be—at the event for which the title Heidegger/Derrida serves as an announcement, the event for which this text—which I, a third author, have signed in advance—is to provide a certain preparation. The concern of course will be not with those two authors themselves, but with the texts that bear their signatures, with what is usually called—quite carelessly no doubt—*their* texts. And so, in the title the names represent, i.e., substitute for and hence announce, certain texts. Or, alternatively, they represent, either directly or through the signed texts, a certain theoretical matter (*Sache*), what is usually called—quite carelessly no doubt—the *meaning* of the texts. Recall the opening sentence of Heidegger's *Nietzsche*:

> "Nietzsche"—the name of the thinker stands as the title for *the matter* [*die Sache*] of his thinking.[1]

The substitutional (representational) function thus traces the relation of author to text and, perhaps less obtrusively, the relation to what is meant by the author in the text: author/text/meaning.

Let me focus for now on the first of these relations, on the way in which the understanding of it has been prepared, most decisively though not exclusively, beginning with Aristotle's *On Interpretation* (16a). What is written (*τὰ γραφόμενα*) is taken to refer to (to be a symbol of: *σύμβολα*) what is spoken (*τὰ ἐν τῇ φωνῇ*); and speech is to be understood by reference to the subject who speaks, as an activity of the speaking subject. An author would perhaps reduce this schema by repressing or merely enacting speech silently; but the relation would remain fundamentally the same, the author producing the text through his activity, expressing in it what is already intact within himself (*τὰ ἐν τῇ ψυχῇ παθήματα*), originating it so as to be entitled to authority over it. Such is, in the merest sketch, the ordinary, i.e., traditional, conception which the texts "of" Heidegger and Derrida serve *to bring thoroughly into question*, both in what they say and often also in their very form of saying, both in word and in deed. Most directly, these texts disrupt the traditional conception by showing how it is inseparable from that metaphysics of subjectivity whose transgression these texts would

*To be presented in an APA symposium on Heidegger/Derrida, December 30, 1984. Edward S. Casey will be co-symposiast; see this JOURNAL, this issue, 601–610. Robert Bernasconi will comment; his paper is not available at this time.

[1] Martin Heidegger, *Nietzsche* (Pfullingen: Günther Neske, 1961), vol. I, p. 9. Trans. by D. F. Krell (New York: Harper & Row, 1979), vol. I, p. xv.

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broach. Thus, in the Heideggerian text, language proves irreducible to the activity of a speaker, requiring rather (to put it in the very briefest formula) a listening to something said in advance of its sounding in human speech.[2] To this extent language is withdrawn from the subject, the author dispossessed of what he would otherwise have taken simply as *his* text. Much the same displacement is released, though from quite a different direction, by certain Derridean texts, for example, by the critique that *Speech and Phenomena* brings to bear upon the Husserlian propensity to conceive expression, meaning, signification, in reference to the τέλος of intuitive fulfillment and thereby to compromise that very originality or ideality of meaning which the *Logical Investigations* was set upon establishing. The conclusion is that signification is essentially capable of functioning independently of all intuition, whether it be that of the sensibly present object or of the self-present subject. The operation of signification, the functioning of language, does not require the presence, even the mediated presence, of the subject; the text is unaffected by the death of the author.[3] In this sense—though only and precisely in this sense—nothing is to be gained by the presence of the author, not even if he were to read his texts aloud, feigning even an originary dictation.

What is put at issue most immediately by the title is not, however, the chain of substitutions linking author to text and to meaning but rather the opposition between the two chains designated by the names of the two authors. Let me focus, then, on the slash. It functions as a kind of punctuation mark, as one of those nonphonetic elements which appear to distinguish writing, to mark it off from speech. If I were to pronounce the title Heidegger/Derrida aloud—for example, at the event for which this text is to provide a certain preparation—if I were to use this text as a pretext for speech, for purposes of that feigning of speech which occurs in reading, then it would be necessary to insert the mark in speech by some kind of artifice, for example, by a gesture that would trace it or by the substitution of a word such as "slash," which would then be functioning in a way quite different from its straightforward semantic function. In living speech, present to its self-present au-

[2] "Speaking is of itself [*von sich aus*] a listening. It is a listening to the language that we speak. Thus speaking is, not at the same time but beforehand, a listening. In an inconspicuous way this listening to language precedes all other kinds of listening. Not only do we speak *the* language, we speak *from out of* it [Wir sprechen nicht nur *die* Sprache, wir sprechen *aus* ihr]." *Unterwegs zur Sprache* (Pfullingen: Günther Neske, 1959), p. 254.

[3] Jacques Derrida, *La Voix et le phènomène* (Paris: Presses Universitaires de France, 1967), ch. VII. Trans.: *Speech and Phenomena*, by David B. Allison (Evanston, Ill.: Northwestern UP, 1973).

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thor, this mark, this trace of writing, cannot be simply *presented*; it remains outside that speech, thus requiring therein its representative, its substitutive gesture or word. And so, the very way in which this mark functions mirrors what will prove to be at issue between those two authorial chains between which the mark is drawn; the issue is presence or, more precisely, withdrawal of, withdrawal from, presence.

Ordinarily this mark—which I have already begun calling by substitution a "slash," translating this artifice back into the written text—ordinarily it is taken as marking some kind of opposition. And yet, a primary strategy of the Heideggerian and Derridean texts is precisely to deconstruct binary opposition. Thus Heidegger's *Nietzsche* proposes, in view of such founding oppositions as intelligible/sensible, an inversion of "Platonism" in which philosophical thinking would also twist free of it[4]—what the Derridean text will designate as the two operations of reversal and displacement. It is a matter of opposites that can be neither reduced to unity nor stabilized hierarchically nor dialectically sublated into a third term. Under the title *Dissemination* it is a matter especially of displacing the three of ontotheology, of destroying the trinitarian horizon, of adding a fourth, self-effacing term which, like *différance*, displaces synthesis, opening the triangle on a fourth side, reinscribing it as an open square.[5]

And so, it is here a matter of preparing such an opening from the opposition of the two authorial chains, in this way preparing for that event to which this text—which I, a third author, have signed in advance—is directed. Or rather, it is a matter of relinquishing this point, this apex, and of beginning to write a fourth text, feigning a preface (*ibid.* 33; tr.: 27). It goes without saying that here it can be a matter *only* of beginning, of merely locating the site for a deconstructive geometry.

"Everything starts with the dyad" (*ibid.* 31; tr.: 24), and so in order to begin it is necessary to return to the opposition Heidegger/Derrida. And yet—remaining for now within the classical conceptuality, e.g., of German Idealism—opposition is never simply such, never opposition as such; even if absolute in form, opposition is never absolute in content but presupposes a conditioning position. The opposition of opposites presupposes a common position, the slash conjoining before it can separate. It is from that position that one must begin.

Let us begin, then, from the positing of metaphysics as at its end, its limit, the delimitation of metaphysics in its closure, a positing

[4] Heidegger, *Nietzsche*, I, 233 (Tr.: 201).
[5] Derrida, *La Dissémination* (Paris: Seuil, 1972), p. 31 f. Trans: *Dissemination*, by Barbara Johnson (Chicago: University Press, 1981), pp. 24 f.

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of it for destruction (*Destruktion*) or—reproducing orthographically the necessary bond of the operation *with* metaphysics—the deconstruction of metaphysics. This positing is governed by a certain exposure of metaphysics, an exposure of a positing common to metaphysics throughout its history and warranting to this degree the phrase 'metaphysics as such'. What is common to metaphysics is the positing of Being (*Sein*) as presence, its self-positing as metaphysics *of presence*.

Hence, it is imperative to understand what "presence" means, to understand how the meaning of presence is such that metaphysics of presence is posed for delimitation, deconstruction. It is with the question of the meaning of presence that one can begin to construct the deconstructive square.

In *Being and Time* presence means predominantly, though not exclusively, *Vorhandenheit* (presence-at-hand, in the usual translation). This is to be understood in its correlation with pure seeing, with νοεῖν,[6] with intuition (*Anschauung*): When something gives itself to one's sheer gaze, when it is simply there for one's looking, displaying itself before and for apprehension, then it has the character of being present-at-hand. Such a character is to be contrasted with that of things with which one deals concernfully, when one manipulates things and puts them to use. The contrast between *Vorhandenheit* and *Zuhandenheit* (readiness-to-hand, in the usual translation) is well known, and the relevant analyses offered by *Being and Time* need not be repeated or even summarized here. What does, however, need to be stressed is the rigorous order that the phenomenological analyses of *Being and Time* (I, 3) establishes with respect to these two modes: Presence-at-hand is founded on readiness-to-hand, and things come to show themselves as present-at-hand only when certain structures of readiness-to-hand get covered over or repressed. One could say, then, that in the strict sense everything is ready-to-hand; or, alternatively, that there is nothing purely present-at-hand. In what one might take as present-at-hand—e.g., the hammer merely stared at—there is always something else operative yet repressed, a concealed operation of readiness-to-hand, a disregarded instrumentality. What is decisive is the displacement of presence that this analysis produces. There are no simply, sheerly present things; for everything is openly or concealedly ready-to-hand, and what is ready-to-hand—the hammer when one takes hold of it and uses it—is not sheerly present as a self-contained positivity. Rather, it is extended beyond itself into the referential totality by which it is determined, its presence limited and yet rendered

[6] Cf. *Sein und Zeit* (Tübingen: Max Niemeyer, 1960, 9th ed.), pp. 25 ff. Trans.: *Being and Time* (New York: Harper & Row, 1962).

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possible by its insertion in that totality. But the totality is one of *signifying* references; it is *Bedeutsamkeit*, the operation of signification.[7] There is no pure presence; for in whatever presents itself there is already in play the operation of signification. Presence is delimited—limited and yet rendered possible—by the operation of signification.

The Derridean text too is addressed to a delimitation of presence, of that sheer perceptual presence that would be correlative to the stratum of pre-expressive experience which Husserlian phenomenology would rigorously separate from the operation of signification. It is on the most elementary level that *Speech and Phenomena* is focused: simple sensory presence (e.g., of a tone) in the living present. That is, it is a matter of deconstructing the phenomenology of internal time-consciousness, bringing fully into play the Husserlian analyses of retention and protention in precisely such a way as to show how that phenomenological text works against itself, withdrawing that very self-identical present, the living present, which, on the other hand, it would preserve as the nondisplaceable center, the living core, of time. Both the Husserlian and the Derridean analyses are well known and need not be repeated here. It is in a sense only a matter of stress, of stressing that the connection of the present to the immediate past and future is an essential, i.e., constitutive, connection. It is not as though there is first a present "now" which then comes to be connected to past and future; rather these connections belong to its very constitution as a present "now"—which is to say that there can be no simple self-identical present. The peculiar complexity of the "now" is elaborated in *Speech and Phenomena* by means of the concept of repetition. The point is that the "now" essentially involves a possibility, structure, or movement of repetition in two senses. First, the present "now" includes repetition of the previous "now," namely in retention. In other words, the present "now" must be constituted in such a way that it is possible for the past "now" to be repeated in it; its constitution must be such as to include such possibility of repetition. Secondly, the present "now," the *form* of presence, is itself ideal and hence infinitely repeatable, its return *ad infinitum* being "inscribed in presence itself."[8] Thus, the present "now" essentially involves

[7] "The relational character which these relationships of assigning possess, we take as one of *signifying* [*be-deuten*]. . . . The relational totality of this signifying we call *significance* [*Bedeutsamkeit*]" (*ibid.*, 87). The more phenomenological formulation of the results in §18 has usually been emphasized: Things can present themselves only from within a world whose structure (worldhood) is a referential totality. This formulation has the disadvantage of not underlining so forcefully the deconstructive character of the analysis.

[8] *La Voix et le phénomène*, 75 f. (Tr.: 67).

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the possibility, structure, movement of repetition both *of previous "nows"* in itself and *of itself* as the ideal form of presence.

This figure of repetition may be called the *trace*. It is, as it were, the figure into which the phenomenological figure of closed-off presence gets deconstructed:

> Such a trace is—if we can employ this language without immediately contradicting it or crossing it out as we proceed—more "primordial" than what is phenomenologically primordial (*ibid.*).

Or again:

> In all these directions, the presence of the present is thought of as arising from the bending back of a return [*du pli du retour*], from the movement of repetition, and not the reverse (*ibid.*).

This operation of the trace, this bending back of a return by which the presence of the present is constituted—to employ still, under erasure, the language of phenomenology—this also is called *différance*. Or it may be called the operation of signification, taking the latter in the most deconstructively (not phenomenologically) primordial way. As in the Heideggerian text, presence is delimited—limited and yet rendered possible—by the operation of signification. The latter is not a matter of presence, not even necessarily of presence to or of an origin, a speaker; like writing, its function is independent of the absence, even the death, of the author. Or rather, this operation of signification is writing in that form in which it emerges from the application of the double operation of reversal and displacement to the metaphysical opposition speech/writing. As *archi-écriture* it is "at work at the origin of sense," delimiting presence.[9]

In conclusion, three issues:

(1) Though in *Being and Time* "presence" means predominantly *Vorhandenheit*, this is not the only meaning of "presence" in play. In fact, an entire chain of meanings is introduced at the outset (§ 6) only to be provisionally abandoned in the published portion of the text. But, beginning with the 1927 lecture course, *The Basic Problems of Phenomenology*, and still more openly in later texts, a system of meanings of presence is developed along various, not easily reconcilable directions. A note in the Derridean text

[9] "This arche-writing is at work at the origin of sense. Sense, being temporal in nature, as Husserl recognized, is never simply present; it is always already engaged in the 'movement' of the trace, that is, in the order of 'signification.' It has always already issued forth from itself into the 'expressive stratum' of lived experience." *Ibid.*, 96 (Tr.: 85 f.).

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which is itself characterized as a note on a note from *Being and Time* calls attention to this issue, this problem of translation:

> The following pages may be read as timid prolegomena to a problem of translation. But who better than Heidegger has taught us to think what is involved in such a problem? Here, the question would be the following: how to transfer into, or rather what transpires when we transfer into the single Latin word *presence* the entire differentiated *system* of Greek and German words, the entire *system of translation* in which Heideggerian language (*ousia, parousia, Gegenwärtigkeit, Anwesen, Anwesenheit, Vorhandenheit,* etc.) is produced? . . .Above all, how to transfer into the single word *presence,* both too rich and too poor, the *history* of the Heideggerian text which associates or disjoins these concepts in subtle and regular fashion throughout an itinerary that covers more than forty years?[10]

(2) On the other hand, the question is whether the Heideggerian text, moving in the system of translations of presence, succeeds in delimiting presence as such:

> The Heideggerian de-limitation consists sometimes in appealing to a less narrow determination of presence from a more narrow determination of it, thereby going back from the present toward a more original thought of Being as presence (*Anwesenheit*), and sometimes in questioning this original determination itself, and giving us to think it as a closure, as *the* Greco-Western-philosophical closure. . . . In the first case the displacements would remain within the metaphysics of presence in general (*ibid.* 75; Tr.: 65).

Does this first case really "occupy *almost* the entirety of Heidegger's text"? Or, on the contrary, is there perhaps always in play in the Heideggerian text that directedness, announced in the 1927 lecture course[11], the directedness ἐπέκεινα τῆς οὐσίας, beyond to an order that is not a matter of Being, of presence, beyond to the meaning, the truth, the clearing of Being, beyond to that which de-limits Being as presence?

(3) Finally, it is necessary to disrupt a web of assumptions that I have left undisturbed hitherto, most pointedly, the assumption that in asking about the meaning of presence one is asking simply for one or several positive senses. It is necessary to disrupt this assumption of straightforwardness because meaning "has never been con-

[10] *Marges de la philosophie* (Paris: Minuit, 1972), pp. 35 f. Trans.: *Margins of Philosophy*, by Alan Bass (Chicago: University Press, 1982), p. 33. The issue is raised again near the end of "Ousia and Gramme": "What about presence then? We cannot easily think in the Latin word *presence* the movements of differentiation that are produced in the Heideggerian text. The task here is immense and difficult." *Ibid.*, 75 (Tr.: 64).

[11] *Die Grundprobleme der Phänomenologie* (Frankfurt a.M.: Klostermann, 1975), pp. 402–404. Trans.: *The Basic Problems of Phenomenology*, by Albert Hofstadter (Bloomington: Indiana UP, 1982), pp. 283–285.

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ceivable, within the history of metaphysics, otherwise than on the basis of presence and as presence" (*Marges de la philosophie* 58; Tr.: 51)—as, to take the exceptionally revealing Husserlian case, presence to eidetic intuition. To ask straightforwardly about the meaning of presence is already to assume what presence means, to assume it in the very question of meaning.

But then, is not the Heideggerian question itself caught in this web of assumptions and as a result held firmly within the closure of metaphysics? If the question of Being is determined as a question of meaning, as the question of the meaning of Being, then is it not in its very formulation a question of presence, a question directed toward a recovery of presence? Indeed this would be so, *were it not the case that* the Heideggerian text, from *Being and Time* on, engages ceaselessly in a deconstructive reduction, a delimitation, of meaning, its reduction to the *woraufhin des Entwurfs* (*Sein und Zeit*, 151), its referral to world, i.e., signification, and eventually to ἀλήθεια. Meaning as presence becomes, is reduced to, the meaning of presence, the latter taken not straightforwardly, but as that which delimits presence. The Heideggerian text, thus releasing the torsion in the question of the meaning of presence, twists it free of metaphysical closure.

Is it not in this way, through such distortion, that the Heideggerian text could be twisted together with the Derridean text, the authorial chains intertwined in a new contortion which, broaching the delimitation of what one would like to call presence *as such*, would form the site of the opposition Heidegger/Derrida?

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ORIGIN(S) IN (OF) HEIDEGGER/DERRIDA*

Everything begins by referring back, that is to say, does not begin.— Jacques Derrida

IT would doubtless be far more fashionable at this time to take up the issue of ends, above all the end of philosophy, in the work of Heidegger and Derrida. Yet although the two thinkers are in considerable accord when it comes to final things—to matters of finality and especially to finalisms of all sorts—they are

*To be presented in an APA symposium on Heidegger/Derrida December 30, 1984. John Sallis will be co-symposiast; see this JOURNAL, this issue, 594-601. Robert Bernasconi will comment; his paper is not available at this time.

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much more instructively divergent when it is a question of origins. They are quite differently disposed toward problems of origination: in art, in language, and in philosophy itself. So much so that we may conveniently regard them as engaged in a conversation on the very subject of origins. Of such a dialogue Heidegger has written that "basically, there are only two possibilities: either to go to their encounter, or to go counter to them."[1] Here I shall go to their encounter.

It is a salient fact that whereas Heidegger seeks out the origins of entire domains of human activity—say, of modern technology in "enframing" operations of various kinds—Derrida approaches the matter of origins only as it arises in reading particular written texts: e.g., Husserl's "The Origin of Geometry" or Heidegger's own "The Origin of the Work of Art." Whereas Heidegger allows for nontextual events to be (at) the origin, Derrida concerns himself exclusively with *already inscribed* engenderings. More drastically still, Derrida maintains that outside a text there "is" no origin—historical, metaphysical, or transcendental—if by "origin" is meant something having the kind of determinate presence often attributed to veridical perception. Just as "there never was any 'perception'"[2] once we deconstruct the idea of such presence, so there never was any origin either—*except in (and as) a text.* To deny that origins are text-generated (as well as text-deconstructed) is tantamount to turning them into transcendental signifieds, whose claim to extratextual validity is the ultimate, or rather the very first, ruse of Western metaphysical reason.[3]

I

It would be wrong, however, to infer that Derrida lacks interest in origins. He passionately pursues them—even, as we shall see, clandestinely commemorates them—so long as they are placed under the suspension of double marks (quotation marks, parentheses, and those crossings-out which are a legacy from Heidegger). But it would also be mistaken to hold that Heidegger is committed to simple, pre-textual origins. For him, words are themselves origins, as is witnessed by his prolonged meditations on *logos, moira,* and *aletheia.* Further, the very notion of origin is nonsimple in Heidegger's view. More an abyss than a ground or principle, it lands

[1] Martin Heidegger, *What Is Called Thinking?*, trans. J. Glenn Gray (New York: Harper, 1968), p. 77.

[2] Jacques Derrida, *Speech and Phenomena*, trans. David Allison (Evanston, Ill.: Northwestern UP, 1973), p. 103.

[3] On the idea of the transcendental signified, see Derrida, *Of Grammatology*, trans. Gayatri Spivak (Baltimore: Johns Hopkins, 1974), esp. p. 73: such a signified is "*always already in the position of the signifier*" (his italics), thus is textually inscribed from the outset.

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us inevitably in paradox.[4] As a "founding leap" (*Ur-sprung*), an origin is noncontinuous; and it may even arise in the form of "strife" (*Streit*) between elemental factors: for instance, between "world" and "earth" in a work of art. The strife itself—a manifestation of a primal "dif-ference" (*Unter-scheid*)—is generative and not merely divisive.

There is difference—also named "rift," "retreat," "withdrawal"—at the Heideggerian origin. Nevertheless, however intrinsic it may be to the process of origination, difference is not the last word. The ur-strife between earth and world is said to attain "the simplicity of intimacy" and even a state of "repose," the two opponents being carried "into the source of their unity by virtue of their common ground."[5] A strife-born origin is reunified out of the intensity of its own internal conflict.

It is this moment of assured reunification which calls out Derrida's sardonic *skepsis*. For it represents the long shadow of metaphysics still darkening the thought of someone as trenchantly antimetaphysical as Heidegger himself. Derrida detects the same shadow falling on Heidegger's unexamined phonocentrism, on his all too Platonic notion of "fallenness," and on his privileging of authenticity and nearness. But it is Heidegger's allegiance to reunification-within-the-origin which is most distressful—and most demanding of deconstruction. The same allegiance is revealed in the continual Heideggerian recourse to words prefixed by '*ge-*', which connotes a gathering or grouping: e.g., *Geviert, ge-eignet, Ge-stell, Gemüt, Gedächtnis, Geschick*. '*Geschick*' is especially noteworthy in the present context; it refers to the way that Being originates in epochs: *das Geschick des Seins* is a "destinal" sending of Being from a gathered origin.

But what, asks Derrida, if there is no assurance of such an origin—no gathering before (and even as) a *Geschick*, no "repose," no "common ground"? What if the origin, rather than being a "grouped indivisibility,"[6] is always already divided from itself—so unredeemably that there is no prospect of reunification except by means of a metaphysical masquerade? What if all begins in difference without hope of "unity"; or more exactly in *différance*, which in its differing/deferring action "gathers itself only in dividing it-

[4] Hence the circle posed at the opening of "The Origin of the Work of Art": "the artist is the origin of the work. The work is the origin of the artist. . . art is the origin of both artist and work" (reprinted in *Poetry, Language, Thought*, trans. Albert Hofstadter (New York: Harper, 1975), p. 17.

[5] See *ibid.*, pp. 49, 50, and 63, respectively for these citations.

[6] Derrida, "Sending: On Representation," trans. Peter and Mary Ann Caws in *Social Research*, II., 2 (Summer 1982): 322.

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self" (324)? What if such a *différance* at the place of the origin (and *taking the place* of origin) is "the condition for there being a *Geschick*, even a *Geschick* of Being, a dispensation of Being and time, of the present and of representation"?[7]

II

And of representation: the issue of origins comes to a head in dealing with representation, "the metaphysical basis of the modern age."[8] According to Heidegger, representation of every sort is a matter of a subject's setting-before (*vor-stellen*) himself or herself an object (*Gegen-stand*) conceived as standing-over-against this same subject. Representing thus construed is in turn subsumed under the more general operation of "enframing" (*Ge-stell*), "that setting-upon which sets upon man, i.e., challenges him forth, to reveal the real, in the mode of ordering, as standing-reserve."[9]

It is altogether characteristic of Derrida to launch a deconstructionist reading of Heidegger by posing a question of translation: is "*repraesentatio*" adequately captured by "*Vorstellung*"? This is not merely "one problem of translation among others; it [raises] the problem of translation itself."[10] It raises the latter problem insofar as translation is thought to be a mere matter of representing an identical, unchanging sense in two (or more) natural languages (see p. 297). Such a view of translation presumes that there is: (a) such a thing in general as a "minimum and shared semantic kernel" (298); (b) such a kernel for the word "representation" in particular. Derrida questions both presumptions: on the one hand, the very notion of a semantic core is only one more instance of a transcendental signified, feigning to stand free from the aberrant itineraries of signifiers; on the other hand, the multiple meanings of 'representation' in its diverse epistemological, aesthetic, political, etc., acceptations defies reduction to a single operation of *vor-stellen* just as it resists synthesis under anything comparable to the gathering action of *Ge-stell*.

We are left with the irreducibility of the '*re-*' such as it is also found in other key words (e.g., *répétition, remarque, retrait, renvoi, restance*: these constituting a veritable *re-* series comparable in import to Heidegger's *ge-* group). Derrida fastens on this prefix and performs upon it a "double gesture": at once extracting it from the paleonym *repraesentatio* and grafting it back on with the extended

[7] *Ibid.* I have modified the translation slightly.
[8] *What Is Called Thinking?*, p. 83. Heidegger adds that this age "is not ending now but only just beginning" (*ibid.*).
[9] *The Question concerning Technology*, trans. Wm. Lovitt (New York: Harper, 1977), p. 20.
[10] "Sending: On Representation," p. 302.

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meaning of repetition.[11] *Repraesentatio* is ineluctably *re-praesentatio*—not as the mere reappearance of an originary (and now lost) (because past) presence, but as a generative act of repetition that "precedes" presence itself. In a word, "the presence-of-the-present [which is embodied in representations of all sorts] is derived from repetition and not the reverse."[12] As repetition is the production of difference within the same—and not at all a return to the strictly self-identical—the "originary" activity of representation as re-presentation is disseminative rather than gathering or unifying.[13]

With this programmatic point in hand, Derrida can add that Heidegger's epochal interpretation of the age of representation as derivative from the era of Greek presence—however admirably indirect this derivation may be[14]—itself depends on an unanalyzed model of representation as delegation-of-power. This lands Heidegger in an embarrassing ontological circle: representation (qua enframing or picturing) relies upon an onto-genesis or *Seinsgeschick* which can be understood only on the basis of another sense of representation, which is left unclarified in Heidegger's text.[15] The deconstructionist tactic is familiar: the subordinate term (i.e., *repraesentatio*) of a metaphysically determined binary opposition—a term supposedly derivative from the dominant term (presence as *Anwesenheit*)—is shown to be subversively operative within the opposition itself, being its secret motor and being even determinative of the dominant term, as in the comparable case of speech/writing. It is less a matter of the return of the repressed than of the insidious power of the repressed itself to undermine the repressing force from within: "representation" undermines "presence" at (and in) its own epochal game.

III

No one gets away scot-free in the deconstructionist enterprise, not even the deconstructor. *Tua res agitur*: Deconstruction can be turned back against itself—in fact, Derrida has just demonstrated

[11] On the notion of "double gesture" (also called "double writing" and "double science"), see Derrida, *Positions*, trans. Alan Bass (Chicago: University Press, 1981), pp. 41, 65; on extraction-grafting-extension, see *ibid.*, p. 71.

[12] *Speech and Phenomena*, p. 52. This principle derives from the rule that "absolute ideality is the correlate of a possibility of indefinite repetition" (*ibid.*).

[13] On re-presentation as "originary" and as "older" than presence, see p. 103.

[14] On this point, see *The question Concerning Technology*, p. 39: a given *Geschick* "does not allow itself either to be logically and historiographically predicted or to be metaphysically construed as a sequence belonging to a process of history."

[15] "It [is] difficult to avoid the question whether the relationship of the epoch of representation to the great Greek epoch is not still interpreted by Heidegger in a representative [i.e., representational-as-delegational] mode, as if the couple *Anwesenheit/repraesentatio* still dictated the law of its own interpretation, which does no more than to redouble and recognize itself in the historical text it claims to decipher" ("Sending: On Representation," p. 322).

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this in the case of Heidegger, himself a masterful deconstructor *avant la lettre*. It would not be difficult to show Derrida dismantling himself in turn, and in the very present instance. Must he not presuppose some core sense of 'representation' after all, if the critique of Heidegger as a crypto-representationalist is to work? If there is no such core or set of connected cores—if it is entirely a matter of "semantic mirage" (*Positions*, p. 6)—how can "representation" be coherently invoked in holding that Heidegger is making a suspicious use of it? To make his argument work, Derrida has to allow at least one meaning of 'representation' to be held constant, to wit, that of representation-qua-delegation. *This* meaning must perdure in and through the juxtaposition of the deconstructing and the deconstructed texts—or else their *Nebeneinanderstellung* would be senseless indeed. Either such a perduring meaning is truly exceptional (requiring a special plea that Derrida does not make) or it is altogether normal [thereby calling into question Derrida's general project of subverting the very idea of an "invariable identity of sense" ("Sending," p. 303)]. One cannot have it both ways. Much the same problem is evident when the critical deconstructionist claim is made that "the idea of repetition and return . . . resides in *the very meaning* of representation" (*ibid.*, p. 308; my italics). Surely "the very meaning of representation" implies a constant semantic kernel akin to what Husserl would term, in his logocentric audacity, the "noematic nucleus" of sense.

The deconstructability of a deconstructionist is a much graver matter than, say, the psychoanalyzability of a psychoanalyst. The latter surprises no one, and would even be welcomed as salutary by any honest psychoanalyst. But an explicitly deconstructionist text aspires to a degree of self-probity and self-transparency that should leave no undeconstructed residue—at least none that would put into question a principal claim of that same text. Yet the fact is that Derrida cannot dispense with a notion that he has himself gone to pains to deconstruct. There is in Derrida's text, if not a return of the repressed, a deferred display of the deconstructed.

IV

If 'representation' as employed by Heidegger has thus been rather thoroughly deconstructed—and this same deconstruction seen as subject to an auto-deconstruction within the deconstructing text—with what are we left? Derrida's answer is: *renvois*, pure referrings-back, "referrals."[16] Of these Derrida says, "as soon as there are *renvois*, and it is always already, something like representation no

[16] I am indebted to Mary C. Rawlinson for this suggested translation of *renvois*. I would also like to thank Véronique Fóti for allowing me to read her insightful essay, "Representation and the Image: Between Heidegger, Derrida, and Plato" (forthcoming in *Man and World*).

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longer waits [as in Heidegger's epochalism], and we must perhaps arrange to tell this story differently, from *renvois* of *renvois* to *renvois* of *renvois*, in a destiny which is never certain of gathering itself up, of identifying itself, or of determining itself" ("Sending," p. 325). In the perpetual deferment of *différance*, there are only traces making continual referrals—*not* references—to other traces.

Rather than worry about the barely suppressed metaphysics at play here (especially when the tracing action of the *renvoi* is said to be "the condition" (324) of Heideggerian *Geschick*), I would like to take up just one aspect of the deferring implicit in the *renvoi*: namely, its memorial, and still more specifically its commemorative, dimension. The issue of origins here becomes that of how (if at all) origins are to be commemoratively recalled. If "everything begins by referring back (*par le renvoi*), that is to say, does not begin" (*ibid.*), everything is in effect a memory trace—but of what? If not of a beginning, then of an (absent) origin. Everything begins by forgetting this origin:

> The beginning of Western thought is not the same as its origin. The beginning is, rather, the veil that conceals the origin—indeed an unavoidable veil. . . The origin keeps itself concealed in the beginning (*What Is Called Thinking*? 152).

On this view, when we think back to the origin(s) of Western philosophy, we think back not to an event in history (i.e., a beginning point) but to an event of withdrawal—ultimately the withdrawal of the ontological difference between Being and beings. Thinking back in this way—even though it is recalling a moment of oblivion—is for Heidegger a commemorative act, an act of *Andenken*. Otherwise put, in practicing *Andenken* we commemorate the trace (in a text) of the (withdrawal of the) trace of the ontological difference.

Nowhere is Derrida's acid skepticism more manifest than just here. Not only has the ur-trace of the ontological difference disappeared (as Heidegger acknowledges), but the very trace of this trace has vanished as well. Where Heidegger locates (and commemorates) the trace-of-the-ur-trace in, say, a fragment of Anaximander, Derrida will claim that the ur-trace has vanished *without a trace* in the pre-Socratic text. As against Heidegger's view that a text can gather traces that preserve forgotten traces, Derrida contends that, considered as a pure *renvoi*, no trace preserves (anything: any memory of any event, including an event of withdrawal). Far from making preservation possible, Anaximander's text exhibits "the *absolute erasure* of the 'early trace' of [the ontological] difference."[17]

[17] "Ousia and Grammē: Note on a Note from *Being and Time*," trans. Alan Bass, in *Margins of Philosophy* (Chicago: University Press, 1972), p. 66; my italics, See also p. 23.

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Such is the direct consequence of a doctrine of textual dissemination: even if a given text contains *renvois* to a concealed "origin," these referrals in no way sustain this putative origin or its concealment. Recourse to commemorative thinking becomes pointless when origins are regarded as so hopelessly irretrievable. Hence the conspicuous omission in Derrida's writings of anything like *Andenken,* which was for Heidegger an increasingly indispensable notion.[18] With the removal of any fore-gathering in an origin or in its traces comes a commensurate elimination of any after-gathering in commemoration.

V

For both Derrida and Heidegger, the metaphysical tradition is a massive, inescapable fact; but their responses to this shared perception are revealingly different. Heidegger believes it both possible and desirable to go behind this tradition by commemorating its withdrawn origins in traces left in "early words concerning Being."[19] Derrida holds out no such hope for circumvention and return; the *renvoi* can only gesture emptily back toward "origins" that seem more mocked than honored in the enclosure of their double marks. For we cannot commemorate what has not been deposited—gathered and preserved—in textual traces. In Derrida's alternately despairing and exhilarating vision, a text, far from being an apt vehicle of commemoration, effaces the signs of its own generation. What Derrida says of the Heideggerian text he would also say of the text of Western metaphysics as a whole: it is "impossible to get around, for others and for myself."[20] One cannot get *around* a text so as to locate its extratextual origin because in the very effort to do so one meets yet another text; any *soi-distant* "origin" is always already pre-inscribed in a text. In short, there is only the re-inscription of one text in another, the re-marking of one by the other—the tracery of intertextuality.

Here we must ask: in dealing with origins, is the choice limited to a nostalgic *Andenken* on the one hand and to an endless round of re-inscriptions on the other? I think not. Overlooked by Heidegger and Derrida alike is a third possibility, which I should like to call "transcription," a word I borrow from Freud.[21] In transcription—as in its psychotherapeutic analogue, "transference"—com-

[18] Thus Heidegger came to claim that "Denken ist Andenken": "thinking is commemorating." [This is from *Was Heisst Denken?* (Tübingen: Niemeyer, 1961), p. 159; the statement is omitted from the English translation.]

[19] *Early Greek Thinking,* trans. David Krell and Frank Capuzzi (New York: Harper, 1975), p. 51.

[20] "The *Retrait* of Metaphor" in *Enclitic,* II, 2 (Fall 1978): 10.

[21] See Letter 52 to Fliess (Dec. 6, 1896) for the notion of a continual "*Umschrift*" within the physical system.

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memoration is at work, even if in no very visible manner. Indeed, unlike *Andenken*, such commemoration is often covert, since there may be no express remembrance of origins or tribute paid to them. Here we can concede to Derrida that "origins"—at least in philosophy—always come inscribed in texts even when these same texts do not refer to them explicitly. Instead of being restricted to sheer *renvois*, however, a transcriptive text can carry over (*trans-ferre*) an "origin" from a past text into its own textual space. Moreover, within such a space we are not constricted to an alternative of preservation or of re-inscription, each of which is static and inherently mummifying, a matter of ingathered indivisibility.[22] Precisely as *trans*criptive, the successor text is transformative; not being confined to repeating and re-marking, much less to gathering and preserving, it effects changes in the text of "origin" itself. This transformation—not to be confused with any merely progressive *Aufhebung*—is wrought by the transcriptive text's commemorative activity.

Just as the death of loved ones can transform them into active internal presences in the quietly commemorative working-through effected in mourning, so philosophers can transcribe their fading or lost traditions, not excluding the metaphysical tradition, in ways that are at once continuously commemorative and yet quite self-critical. And as internalization has to be distinguished from mere incorporation in the intrapsychic realm,[23] so transcription has to be distinguished from *Andenken* and re-inscription, both of which operate by the incorporation of paleonyms. If "the ultimate business of philosophy is to preserve the force of the most elemental words in which Dasein expresses itself,"[24] it is part of the same enterprise to transform these words as well as to preserve and re-textualize them. It is a matter of letting the paleonyms resonate in new registers of their own "original" significance, rather than seeking nostalgically for their pre-metaphysical moorings or incorporating ("extracting") them for use as "levers of intervention."[25]

Of course we are not restricted to mourning—helpful as this may

[22] "In folding it back upon itself, the text thus *parts* (with) reference" (Jacques Derrida, *Dissemination*, trans. Barbara Johnson [Chicago: University Press, 1981], p. 270; his italics).

[23] On internalization vs. incorporation, see Hans W. Loewald, "On Internalization," in *Papers on Psychoanalysis* (New Haven: Yale, 1980) pp. 69–86; and Roy Schafer, *Aspects of Internalization* (New York: International Universities Press, 1968), *passim*.

[24] Martin Heidegger, *Being and Time*, trans. J. Macquarrie and Edward Robinson (New York: Harper, 1962), p. 262; Heidegger underlines "force of the most elemental words."

[25] On extracting and the lever of intervention, see *Positions*, p. 71.

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be as a model[26]—for pertinent instances of transcription. Such instances lie open before us in the writings of Heidegger and Derrida themselves. For what reunites these two dissident thinkers and justifies their co-nomination as "Heidegger/Derrida" is a common capacity for creative transcription of certain philosophemes in the Western tradition. Consider only the way that in Heidegger's text "presence" (*Anwesenheit*) transcribes *ousia,* "aspect" (*Gesicht*) conveys *idea,* and "letting-lie-together-before" (*beisammen-vor-liegen-Lassen*) translates *legein.* Here "translation" indeed becomes "transformation" (*Positions,* p. 20).

Or gaze finally at '*différance*', a word which literally (and silently) in-scribes 'difference'—the difference of Hegel and of Heidegger himself—into its internal structure. 'Difference' is also trans-scribed; it becomes other than itself by its conjunction with deferring, itself a transcription of Freud's notion of deferred action (*Nachträglichkeit*) and Levinas' idea of the belated trace. The composite word '*différance*' thus holds together—in one congenial gathering—Hegel and Heidegger, Freud and Levinas: com-memorating them despite its own divisive action. Instead of the decomposition of deconstruction we witness the re-composition realized in commemorative re-collection. If '*différance*' does not "represent" these figures, much less refer to them, it nonetheless does remember them. It, this late/new word, does what Derrida himself, as tactician of deconstruction, disdains to do: it commemorates these very particular "origins" in the transcription accomplished by its own tracing.

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[26] Derrida has addressed issues of mourning and memory in recent (1984) lectures entitled "Mémoire(s)," written in commemoration of Paul de Man.

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AMERICAN PHILOSOPHICAL ASSOCIATION
EASTERN DIVISION

Abstracts of Invited Papers to Be Read
at the Eighty-first Annual Meeting

DUNS SCOTUS ON THE GOODNESS OF GOD*

Medieval theology is a rich treasure of deep insights about good and evil and the meaning of life. In this paper, I appropriate some of it, by drawing from Duns Scotus's account of divine goodness, answers to the question of how an omnipotent God can be seen as wise and good to created persons in a world such as ours.

(1) *Evil and Divine Justice*: Distinguishing natural from moral goodness, Scotus begins with the observation that God as an infinite being is an infinite (natural) good, whereas everything else actual or possible is a finite (natural) good. Since God acts by free choice and is motivated by an *affectio iustitiae* (i.e., an inclination for a disinterested love of goods for their own sake), Scotus infers that God ought to love Himself above all things and for His own sake. But no finite good can coerce the free choice of God or even a creature. Scotus concludes that God has no obligations to any creatures and hence cannot be unjust by violating them. In relation to creation, God has at most an obligation to His own goodness, to be true to Himself and not be a sloppy craftsman.

If it is metaphysically impossible for God to be unjust to created persons, one can nevertheless inquire about the character of His policies and choices toward them. Scotus claims that God is the most well organized of lovers: i.e., that God loves Himself and creation and that God orders the priorities of His loves and chooses the objects of His action in a wise and orderly way. We may thus ask whether any actual or possible created state of affairs is logically incompatible with divine love and wisdom.

(2) *Evil and God's Love for the Elect*: According to Scotus, God has a disinterested love of Himself as the infinite good, and is thereby motivated to create others to share in that love. Some possible creatures He loves enough to create, and of these the elect are contingently predestined to enjoy a beatific vision forever. The evils of sin and suffering are neither among God's principally intended ends nor a logically or metaphysically necessary means to or consequence of it. On the contrary, the beatific vision is a good which so far transcends anything that any created nature needs or requires as to be logically and metaphysically independent of any natural or temporal goods and deprivations. Nevertheless, God's generosity toward the elect, in inviting them to share in the inner life of the Trinity—is immeasurable and hence undiminishable by His permission of evils.

(3) *Evil and Divine Wisdom*: If the evils of sin and suffering in no way impugn God's love for the elect, could they not raise a question about His

*Abstract of an APA invited paper, to be presented December 29, 1984. Eleonore S. Stump will comment.

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wisdom? For if God's main purpose in creation is to have co-lovers who share in the beatific vision and if there is no logically or metaphysically necessary connection between the good of such intimacy with God, on the one hand, and the evils of sin and suffering on the other, is it not a waste for God as well as a pain for the elect to make them pass through this temporal vale of tears?

For Scotus, the natural incommensurability between finite goods and deprivations in nature and the beatific vision does not tell against God's wisdom, because in His wider purpose He has established a connection of "fittingness" between them. Christian revelation shows that God loved temporal natures so much that He wanted to make some of them real and to raise them above their natures by uniting them to the eternal. Thus, He freely and contingently chose to make the soul of Christ fit for glory by joining it to God the Son in hypostatic union and to deem angels and other souls acceptable on the basis of merit. According to Scotus, merit is not a property that pertains to an act by any natural necessity, but is conferred by free and contingent divine legislation that relates the acts of finite and temporal persons to their eternal destinies. God would have established such connections quite apart from whether or not anyone sinned, but, given the entrance of sin and suffering into the world, God is able to integrate them into His order of salvation and thereby give eternal significance to the temporal experience of created persons.

(4) *The Surd of Reprobation*: Scotus' response to the problem of evil as sketched thus far relies heavily on his appreciation of the infinite natural goodness of God, on the one hand, and God's immeasurable generosity toward the elect, on the other. Yet, this very emphasis serves to darken Scotus' admission that God creates some persons whom He fails to raise to glory. Granted that finite persons have no right to anything from an infinite being, how could generosity so great as to create and raise some to eternal glory, consign others to eternal misery by simple omission?

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ENGINEERING ETHICS AND MORAL THEORY*

The philosophical analysis of concrete cases and issues that arise in the general area of engineering has provided and will continue to produce valuable insights into a number of topics—both metaethical and normative—that have been or should be of interest to moral philosophers.

This paper outlines the ways in which work in engineering ethics is directly relevant to the study both of basic metaethical questions and of normative issues. One of the metaethical topics is the relation between

*Abstract of a paper to be read at an APA invited panel on The Contribution of Applied Ethics to Moral Theory, December 28, 1984. Co-panelists will be K. Danner Clouser, Kenneth E. Goodpaster (for his contribution see this JOURNAL, this issue, 616/7), and Richard M. Hare.

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what are today commonly referred to as "theoretical" ethics and "applied" ethics. Although engineering is still often described as essentially identical with "applied science," careful study of the kinds of work actually done by engineers indicates that this is at best a gross and misleading oversimplification. Moreover, the much more complex relation that in fact exists between scientific theory and engineering practice is similar in significant ways to the relation between ethical theory and the making of concrete decisions in everyday life. Thus, the study of the science/engineering relation sheds valuable light on the ethical theory/concrete decision relation as well as on the general question of the epistemological status of ethical theories and concrete moral judgments.

The analysis of ethical problems arising in the practice of engineering requires consideration of every topic in normative ethics, but several are of particular significance and some involve approaching old issues in new ways. One example discussed in some detail in this paper is the concept of informed consent. Engineers as professionals, like physicians and lawyers, perform acts that affect the health and welfare of other people. However, unlike doctors and lawyers, most of the time the work of engineers affects large numbers of individuals over extended periods of time. Also, the affected parties are seldom "clients" in the strict legal sense of standing in an explicit contractual relation to the engineer. The analyses of the complexities of such situations—*who* should be informed? *what* information should be given? *how much* cost and effort should be committed to communicating the information? *how* does one determine whether the information is actually fully understood? *how many* of the potentially affected parties must give consent? etc.—increase our understanding of the notion of informed consent itself as well as of the relative strengths and weaknesses of various normative ethical theories. The examination of these problems also provides insights into the understanding of more abstract and general metaethical and epistemological questions.

Other topics discussed in this paper include the concept of pro bono services, the problem of assigning moral responsibilities to individuals on team projects, and a method of "retrospective" analysis to identify points at which no-win dilemmas (as in whistle-blowing cases) might have been avoided by different decisions earlier in the temporal process.

ROBERT J. BAUM

University of Florida

ON THE CONCEPT OF THE ENVIRONMENT IN EVOLUTIONARY BIOLOGY*

Evolution has as its mechanism the differential reproduction of heritable variation. This differential reproduction may have a variety of causes. Of particular interest to evolutionary biologists is the differential reproduc-

* Abstract of an APA invited paper, to be presented December 28, 1984. William C. Wimsatt will comment.

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tion that is due to differential adaptedness to a common environment. This is natural selection and is explainable by the *principle of natural selection* (PNS). The focus of this paper is on what constitutes a common environment.

I argue that the first step in explaining differences in fitness is to partition the variance in fitness into within-environment and between-environment components. In general only (but not all) the within-environment variance will be heritable. But, more importantly, only the within-environment variance will be explainable by the PNS. Once attention is confined to a single environment, the explainable variance in fitness will be explainable in terms of differences in the character states of the biological entities. One hopes to find a strong relationship between the character states and fitness. (In population-genetic models this would be represented by a positive covariance between genotype and fitness.) Put in other words, by focusing on single environments one localizes the differences relevant to fitness within the realm of differences in the biological entities themselves. Obviously, to effect such a partitioning one must be able to individuate environments.

Our problem would be much easier if we could confine our attention to factors exogenous to the evolving population. But not only would that ignore the effects organisms have on their environment (e.g., the effects of predators on prey populations or of plants on their surrounding soil), it would leave unexplained all cases of density-dependent and frequency-dependent selection. In these cases the state of the population itself is an important determiner of fitness within the population. So, although some environmental factors, such as mean annual rainfall, may be exogenous to the population, other factors will not be. It follows that in order to pick out an environment correctly one must first correctly identify the population.

From a population-genetic point of view the biological world is divided into species which are in turn divided into demes. These divisions are based on genetic interchange and descent and are independent of selection. But demes are not, in general, ecologically homogeneous. That is, within a deme different individuals of the same type may experience relevantly different environments. The units of ecological homogeneity (D. S. Wilson's *trait groups*) will, in general, be smaller that demes, and are the populations we must identify.

It may seem that we have constructed a vicious circle: in order to individuate environments one must first subdivide a given population into units of ecological homogeneity; in order to identify units of ecological homogeneity one must find out which individuals experience a common environment. This characterization is circular, but not viciously so. The division of the population into ecologically homogeneous subunits is done in a way that maximizes the covariance of fitness and character state within the subunit. Although there remain some problems having to do with the finitude of biological populations and the role of chance in reproduction, this approach to identifying ecologically homogenous populations does not require a prior identification of environments.

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Given an ecologically homogeneous population, the environment of that population can be defined as all and only those factors (both biotic and abiotic) which are determinants of fitness for all of the members of the population. This definition is applied to a number of cases at different levels of biological organization and its implications for one important controversy concerning group selection are explored.

In earlier papers I have argued that the PNS is an application of the *law of likelihood* and is absolutely central to the theory of evolution by natural selection. This paper shows that not just any application of the law of likelihood will do, it shows how the PNS constrains and so structures natural selectionist explanations. Put another way, the paper shows how a logically impeccable way of treating relations as properties creates grave difficulties for understanding natural selection.

ROBERT N. BRANDON

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DELIBERATING ABOUT THE SEA BATTLE*

Deliberation may sometimes be pointless, but it would be absurd to suggest it always is. Of course people often act better or more efficiently as a result of deliberation. So, as rational agents, we are bound to believe. Yet according to an ancient tradition (T), if determinism is true, there is never point in deliberating. See Aristotle, *De Interpretatione* 9, 18b 26-33: "[If] everything is and comes to be of necessity . . . there would be no call to deliberate or to go to the trouble of considering that if we do this the situation will be so and so, whereas if we don't do this it won't be so and so."

I argue here that T conveys an important truth, but one that came to be transmitted in corrupt forms laying it open to easy refutation. For after Aristotle T was interpreted by some as implying that (U_1) if determinism is true then whether or not a person deliberates the action taken is the same (cf. Alexander of Aphrodisias, *De Fato* XI, 178/9) and by others as implying that (U_2) if determinism is true then the outcome is the same no matter what a person does (cf. Cicero, *De Fato* XII). However, since U_1 and U_2 are *non sequiturs*, it is usually thought that on that account T fails as an objection to determinism; also that the illusion of consequence in U_1 and U_2 springs from confusing conditional with unconditional necessity. But that confusion may not adequately explain T's occurrence in Aristotle, since the nearby context suggests that he was unlikely to have fallen into such an error.

I argue that T can be interpreted as expressing a view (V) that is (i) correct and (ii) no less deadly for determinism than U_1 would be, or U_2, if *it* were correct. V is the position that, if determinism is true, we are never en-

*Abstract of an APA invited paper, to be presented December 29, 1984. John Cooper will comment.

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titled to hold that if an agent had not acted as he did on a given occasion the subsequent situation would have been better/worse than the actual subsequent situation; also, and correspondingly, that we are never entitled to hold that if a prospective agent does not do *X*, things will be better/worse than if he does it. The rest of the paper is devoted to justifying V on the basis of (i) a more precise statement of the general type of determinism relevant to this argument, and (ii) an examination of the ambiguous counterfactual conditional "If *A* had not done *X*, then under the circumstances *S* would not have followed."

SARAH WATERLOW BROADIE

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APPLYING PRACTICE TO THEORY*

The contribution of applied ethics to moral theory should be distinguished from the equally significant contribution of moral theory to applied ethics. Moreoever, both moral theory and applied ethics make distinct contributions to the living out of the moral life.

If ethical theory seeks to *elucidate* or define the moral point of view, applied ethics seeks to provide better *access* to the moral point of view in our daily problem solving and decision making. Three salient arenas in which the latter type of inquiry contributes to the former are those of relevance, scale, and method.

Relevance. The first arena is perhaps the least controversial and for this reason the easiest to undervalue. Applied ethics offers moral theory a host of real-life problems and problem-types to help prevent the kind of irrelevance that often leads to spurious argumentation. By separating reality from fantasy, applied ethics makes it easier for the moral theorist to avoid preoccupation with possible worlds to the neglect of the world in which he or she lives. As William Frankena has reminded us, "morality is made for man, not man for morality."

In the applied domain of ethics in management, for instance, there are innumerable challenges to moral reflection because of the pervasive influence of business activity and large organizations in the lives of each of us. Where there is such pervasive influence, moral thought takes on added value and relevance.

Scale. The second arena has to do not so much with the *problems* as with the *parties* of moral transactions—the agents and recipients involved in personal, organizational, and societal decision making. The contribution of applied ethics here is to identify and relate the various levels of at-

*Abstract of a paper to be presented in an APA invited panel on The Contribution of Applied Ethics to Moral Theory, December 28, 1984. Co-panelists will be Robert J. Baum (for his contribution see this JOURNAL, this issue, 612/3), K. Danner Clauser, and Richard M. Hare.

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tribution (moral units of analysis) that present themselves. Some examples of questions about scale are whether and in what sense corporations, like persons, can be regarded as moral agents and recipients.

The answers to such questions are not simply *logical* truths about moral discourse. They have to do not with possible worlds but with the actual world. Nor are they *ontological* truths about individuals and organizations. The "furniture" of the moral world, like that in one's study, needn't be regarded as ontologically primitive or irreducible in order to be used for sitting, writing, storing information, etc. It serves its purpose if it helps one to get on with the business of living well. The ontological status of persons and corporations can remain open while moral philosophers explore the similarities and differences between personal and corporate responsibility.

The "logic" governing issues of scale is less abstract and more pragmatic than that which governs the meanings and implications of concepts like 'ought' and 'morality'. Thus applied ethics in this arena is a kind of "metaphysics of morals" without the trappings of logical or ontological ultimacy. It helps the moral theorist to appreciate the multidimensional structure of practical problems that call for moral judgment.

Method. The third arena is more controversial than the first and the second. It has to do with the methodology of moral thought. Studying on a case-by-case basis the challenges of decision making under conditions of uncertainty and personal and institutional imperfection can raise difficult epistemological questions for moral theory. Try as one might to achieve positions variously described as archangelic, original, and ideal, all decision making seems in the end to be a matter of "balancing" on the part of the parties involved. Critical thinking in ethics is essential, but accounts of critical thinking must be compatible with the realities of human decision makers. This is a philosophical point, not an accidental side constraint.

There is a strong temptation to think that the solution to the problem of conflicting moral intuitions lies in a promised land in which such conflicts are "transcended" in favor of precision and criterial clarity. The alternative, it is thought, leads to relativism or subjectivism. But decision makers whose problems set the agenda in applied ethics "can't get there from here." Neither can the rest of us. It seems clear, however, that some nonideal methods are better than others and that mature moral judgment is not merely a matter of taste.

Applied ethics can contribute to moral theory by reminding us that moral conflict (like pain) is a part of the human condition and not necessarily something to be avoided at all costs. The most sensible strategy may well be to *manage* moral conflict in a disciplined way rather than to try to *eliminate* it by going to another "location." This does not mean giving up on objectivity, rationality, and truth in matters ethical. But it does mean recognizing our moral historicity, both when it comes to defining the moral point of view and when it comes to "taking" it.

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FEMINIST RECONSTRUCTIONS*

Contemporary western feminism is rooted in the discourses of classical liberalism on the one hand and traditional Marxism on the other. Both of these discourses, however, have been found inadequate to express the demands of women. In this paper, I shall explore contemporary feminist critiques of some of the fundamental assumptions of both the liberal and the Marxist traditions and show how feminists currently are seeking to reconstruct prevailing conceptions of human nature, human knowledge, and human political life.

The earliest western feminists were inspired by the liberal respect for the intrinsic dignity and worth of the human individual and the corresponding liberal call for political freedom and equality. Liberal ideals are grounded in the belief that all human individuals (with a few atypical exceptions) possess a capacity for reason which is the distinguishing feature of their humanity. This capacity for reason, understood in a distinctively liberal way, constitutes the universal essence of human nature; other social or biological characteristics of human beings, such as their class, race, and sex, are conceptualized as merely accidental properties. Liberalism's rationalist and universalist conception of human nature generates distinctively liberal interpretations of the political ideals of freedom and equality: freedom is interpreted as liberty, the preservation of a private realm that is free from state intervention; equality is interpreted as equality of opportunity to pursue individual self-fulfillment. Contemporary feminists have noted that the liberal interpretations of freedom and equality are both inconsistent with each other, since the establishment of genuinely equal opportunities within a market society requires constant state intervention, and inadequate for the fulfillment of women's needs. In probing the inadequacies of liberal political theory, feminists have come to challenge liberalism's underlying conception of human nature. This conception of human nature, and its associated conception of political life, are now claimed to be androcentric and male-biased.

The discourse of Marxism appears more promising for feminism at first sight. Marxism rejects liberal "somatophobia" (disdain for the body) in favor of a political theory whose foundation is a recognition of the labor necessary to satisfy human bodily needs. For Marxists, labor rather than abstract reason is the distinguishing feature of human nature. Labor always has both a physical and a mental aspect, but it takes different forms in different societies. The prevailing organization of labor, or mode of production, is a determining influence on every society, shaping its legal and political institutions, its art and its science, and even the human nature of its members. Contemporary feminists have found this general "historical materialist" approach to be fruitful for understanding the oppression of women. However, they charge that the method has been misapplied by traditional Marxists themselves, who have provided unduly

*Abstract of an APA invited paper, to be presented December 30, 1984. Nancy Holmstrom will comment.

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narrow interpretations of such key concepts as "labor," "production," "class," and "economics" and whose political ideals of freedom, equality, and democracy are correspondingly limited. Specifically, to the extent that women's activity is excluded from Marxist political economy, Marxist discourse too is androcentric and male-biased. Contemporary feminists argue that it is necessary to reconstruct our concepts of human nature and political life in such a way as to reveal their gendered structure. Sex itself must be conceived as a social construction rather than as a biological given.

Contemporary feminists are engaged in creating a new feminist discourse, one that is more adequate to describe women's experience and to articulate women's needs. This discourse is grounded in the daily experience of women, but it does not reflect women's spontaneous or immediate perceptions. Rather it is a discourse that emerges painfully in the process of feminist struggle to reconstruct not just our words but also our world, not just our language but also our lives.

ALISON M. JAGGAR

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DISCOVERING THE FORMS OF INTUITION*

My title is slightly misleading because I focus on Kant's theory of spatial perception. This theory is unusually unpopular with Kant's interpreters. Jonathan Bennett dismisses it with contempt. P. F. Strawson parades the theory as an example of the ills of transcendental psychology. Vaihinger and Kemp Smith find the theory shocking, because it seems to rest on nothing but an unsupported premise that spatial relations cannot be perceived. Worse still, as these critics point out, the claim that space is the form of outer sense is no casual addition, but the foundation for some of Kant's most distinctive claims. We reach a surprising result: the master rested large portions of his philosophy on an undefended and implausible assumption. In opposition to the prevailing wisdom about Kant's folly, I argue that his theory of perception is perfectly reasonable in its historical context and probably correct in some of its more general claims.

Among other difficulties, Kant's predecessors tried to resolve the problem of how we perceive the third dimension. Dr. Molyneux gave the problem its classic formulation:

> For *distance* of it self, is not to be perceived; for 'tis a line (or a length) presented to our eye with its end toward us, which must therefore be only a *point*, and that is *invisible*.[1]

*Abstract of an APA invited paper, to be presented December 30, 1984. Carl J. Posy will comment.

[1] From the *Dioptrika Nova* (1692), quoted in Nicholas Pastore, *Selective History of Theories of Visual Perception 1650–1950* (New York: Oxford, 1971), p. 68.

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Descartes's well-known theory of innate geometry proposed one type of solution. Berkeley's *An Essay towards a New Theory of Vision* took a radically different approach. Its thesis is that we are aware of various distance clues which we are able to use because experience has taught us the regular correlation between these clues and the directly perceived tactile distance. The instability of this position, evident in Berkeley's own writings, did not seem to affect its popularity. Voltaire, Condillac, Reid, and Hartley all accepted it. Descartes's side of the debate also had some formidable champions, including Barrow, Malebranche, and Molyneux himself.

The common, well-supported premise of this controversy is that we cannot directly perceive space by sight. Upon reflection, it can easily be seen that touch can fare no better, so space is not directly perceived at all.

Kant's general theory of perception, which is perhaps clearer in the *Inaugural Dissertation*, contains three elements: the object that affects us, the impression itself, and the perceptual scene. The last two elements need to be distinguished, because, as the well-known difficulty about inverted retinal images made clear, we do not perceive sense impressions. In the *Dissertation* Kant reasoned that the principles correlating features of sense impressions with features of perceptual scenes had to be innate, apparently because he took the sense impressions to cover the entire effect of the object on us. In any case, Kant's theory is that the properties of the perceptual scene are a function of the properties of our sensations and the innate principles, which he calls "forms of intuition." Given that we do not know what the properties of our sensations are, we cannot determine the contribution of the forms of intuition—unless there were some features of perceptual scenes that had no input from sensation. The Transcendental Aesthetic utilizes this discovery procedure and the common assumption that spatial location cannot be registered in sensation, to reveal space as a form of intuition.

Kant's theory of outer sense turns out to be a psychological account, just as his critics claim. I argue, however, that this type of psychology, transcendental psychology, is both a legitimate enterprise in itself and the central project of the *First Critique*.

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CONTRIBUTIONS AND LIMITATIONS OF PROCESS PHILOSOPHY*

The process philosophy of Alfred North Whitehead gave clear and comprehensive expression to the intuitions of many early twentieth-century philosophers in Great Britain, France, and the United States that the concepts of time, change, feeling, value, and transition needed reformulation. Whitehead's own formulations inspired a vigorous following, now with its own journal and professional society. My purpose here is to discuss certain revolutionary contributions of process philosophy, to delineate basic limitations, and to suggest how certain of its limitations might be supplemented in order to fulfill its promise.

(1) Whitehead's reconstruction of systematic or speculative philosophy presented it as a comprehensive hypothesis based on empirical generalizations and tested by rational fruitfulness. The very existence of a system of such excellence as Whitehead's legitimates the enterprise, superseding discussions about whether systematic philosophy is possible. Whitehead's analysis of the difficulties of modern Western philosophy was not that it is dying or that it had proved itself utterly unfounded and in need of deconstruction or a linguistic turn, but that it had got stuck on some bad ideas. There are resources in the tradition for far better ideas, and Whitehead claimed to have built his system from them. A more radical point seen in historical perspective is that Whitehead (and others such as Charles Peirce) provide an alternative to the modernism of both Continental philosophy and Anglo-American philosophy. If philosophical modernism has the traits of foundationalism and an insistence that philosopher's work be self-contained in its intelligibility, justification, and worth, then philosophy as speculative hypothesis is nonmodernist. It arises from, appeals to, and finds its measure in what Whitehead called the "broad range of civilized experience." From the standpoint of modernist assumptions process philosophy seems not to be philosophical.

The limitation to Whitehead's sense of speculative philosophy is that it does not allow for asking the ontological question: why is there something determinate rather than nothing? Yet Whitehead's own principles require that complexities be accounted for in terms of the decisions constructing them, a decision in the ontological case that would lie beyond the very principles to which empirical generalization moves. An alternative theory of creativity is required, one rejecting Whitehead's theism.

(2) Whitehead's structural contribution is a revolutionary model of causation, with its attendant implications for relations, substances, environments, and the like. Briefly put, Whitehead hypothesized that an actualized thing, once it has become past, can then function as a potential in any number of subsequent things. In its present momentary existence, a thing consists in the coming-to-be of its own finished self, employing its own

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creativity to alter and integrate its inherited potentials into its own definite actuality, at which point it becomes yet one more potential for the future. There are two important limitations to this hypothesis, however, despite its advantages for the mind-body problem and ecological relations. The first is that it cannot by itself account for the continuity of agency in an extended action. Such an account would require that the many occasions in an enduring agent be *essentially* linked to one another and that the links between those occasions and environmental factors be matters of mutual *conditioning*. The distinction between essential and conditional connections, I argue, rests on norms, not on physical structures. The second limitation is that Whitehead did not provide an account of the nuanced textures of human life. The reason he did not was not merely inattention but his failure to surmount the previous limitation so that diachronic textures can be taken as essential to human life. Paul Weiss's recent philosophy illustrates what Whitehead should have been able to explain.

(3) Value is an essential notion in process philosophy. At least two accounts of value are required: of how it functions in nature and experience, and what it is that makes something valuable. Whitehead had a thorough functional account of value, and a brilliant but brief and only suggestive account of what makes something valuable. In addition to treating the past as a set of potentials for present action, and the present as a process of deciding, Whitehead thought, in good Platonic fashion, that one explains a state of affairs by showing how it is good, a valuable decision-outcome relative to the potentials. This requires a thoroughgoing treatment of the nature of value, the direction in which I argue process philosophy ought to move.

ROBERT C. NEVILLE

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MORAL REALISM*

There has recently been a revival of interest in moral realism, although this label has been applied to a quite diverse range of views. The sort of moral realism I will be concerned with here holds, among other things, that moral theories ought to be judged by standards comparable to those by which we judge scientific theories, that moral judgments can have truth values in a nonepistemic sense, that moral facts supervene upon natural facts, that there are causal mechanisms for gaining moral knowledge, and that we are able to assess moral "intuitions" critically. How could anyone come to believe such a view? In this paper I attempt to show that some frequently given arguments for rejecting such a view are not sound, and to indicate along the way some positive reasons for thinking such a view tenable.

*Abstract of an APA invited paper to be presented December 30, 1984. Nicholas S. Sturgeon will comment.

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The fact/value distinction seems to stand in the way of this sort of moral realism. Yet the empiricists who gave us this distinction left their revolution incomplete by introducing the instrumental conception of rationality but then continuing to speak as if certain modes of inquiry were uniquely rational, no matter what the inquirer's goals. It now seems clear that although norms play an ineliminable role in any determination of what it is rational to believe, no adequate norms of inquiry are mandated by rationality itself. Neither noncognitivism nor relativism follows, however, for we may reformulate criteria of epistemic warrant in externalist (e.g., reliabilist) terms, so that warrant does not require either our premises or our conclusions to be value-free. We may then naturalize questions about when such warrant might be expected to exist. I argue that a particular theory of human interests and preferences that assigns an explanatory function to intrinsic value could receive a naturalistic vindication of this sort.

This would support a kind of value realism, but moral realism must encompass a theory of rightness as well as goodness. Yet how could moral *rightness* be an explanatory property? I take moral rightness to be a matter of what is rational with regard to intrinsic value when assessed from a certain, social point of view. The property of being socially rational in this sense can play a role in explanations of the evolution of social conduct and norms similar to the role that facts about what it would be rational for a given individual to believe or do can play in explanations of what he in fact believes or does. I attempt to show that mechanisms exist in both cases to press beliefs, actions, and norms in the direction of what is rational with regard to intrinsic value from either the individual or social point of view, although the many systematic and unsystematic imperfections of these mechanisms make possible criticism of individual and social conduct and norms. One consequence of the view taken here is that it is a contingent matter for any individual whether it is rational from his particular point of view to be concerned with what is intrinsically valuable or to do what is morally right. However, the view that moral realism requires that some scheme of values be dictated by reason alone or that moral imperatives be categorically binding seems to me to confuse moral realism with moralistic rationalism.

PETER RAILTON

University of Michigan

NOTES AND NEWS

The 1985 Congress of the Canadian Philosophical Association will be held at the Université de Montréal in Montréal, Québec, from May 28–31, 1985. Papers, of no more than twelve double-spaced pages, are welcome from all philosophers. English-language submissions should be sent *in triplicate* to Thomas Hurka, Department of Philosophy, The University of Calgary, Alberta T2N 1N4 by January 15, 1985.

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At the APA, Eastern Division, in December 1983, the International Hobbes Association (IHA) was formed. The International Board is composed of Howard Warrender, D. D. Raphael, and Quentin Skinner of England; Bernard Willms and Klaus-M Kodelle of Germany; Francois Triçaud of France, and Gershom Weiler of Israel; it is chaired by Martin A. Bertman of the USA. The goal of IHA is to encourage research in Thomas Hobbes in a variety of disciplines, through conferences and, eventually, a journal. At present the *Hobbes Newsletter*, edited by Tim Fuller and Martin Bertman, will be a clearing house for Hobbesiana. Dues or items of interest for Newsletter inclusion should be sent to Tim Fuller, Department of Political Science, Colorado College, Colorado Springs 80903. Queries about organizing conferences, grants, and other matters should be sent to Martin A. Bertman, Department of Philosophy, SUNY, Potsdam, N.Y. 13676.

The Southern Society for Philosophy and Psychology will hold its seventy-seventh annual meeting April 4-6, 1985, at the International Hotel in New Orleans, Louisiana. Additional information may be obtained from: Stephen F. Davis, Department of Psychology, Emporia State University, Emporia, Kansas 66801.

Daphnean Press, formerly Precedent Publishing, Chicago, is pleased to announce its interest in reviewing books for publication in philosophy, history, and political science. Inquiries should be directed to Michael Gross, managing editor, Daphnean Press, 737 No. LaSalle St., Chicago, Ill. 60610.

A symposium on ethics and animal experimentation will be held on November 13, 1984, at California State University in Long Beach. Speakers will include Steve Sapontzis (CSU, Hayward), Bernard Rollin (Colorado State), Dale Jamieson (Colorado), and Lawrence Finsen (Redlands). More information may be obtained from the California State University/Long Beach Philosophy Association, 90840.

The Department of Philosophy at the University of Oklahoma announces the appointment of John Biro as chairman. It is also pleased to announce that Brent Mundy joined the department as an Assistant Professor in the fall of 1983 and that Cindy Stern and James Kostman have been appointed Assistant Professors beginning in the fall of 1984.

Space permitting, the Appendix to the Program and the List of Group Participants will appear in the November issue of this JOURNAL. In any case, they will appear with this Program in the November issue of *Proceedings and Addresses of the American Philosophical Association* and in the printed *Program* distributed at the meetings.

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AMERICAN PHILOSOPHICAL ASSOCIATION
EASTERN DIVISION
EIGHTY-FIRST ANNUAL MEETING

NEW YORK HILTON HOTEL, NEW YORK CITY
DECEMBER 27-30, 1984

PROGRAM

THURSDAY, DECEMBER 27

1:00 P.M.–10:00 P.M. REGISTRATION, *Second Floor Promenade*
1:00 P.M. EXECUTIVE COMMITTEE MEETING, *Madison Suite*

8:00 P.M. SOCIETY OF CHRISTIAN PHILOSOPHERS, *Beekman Parlor*
8:00 P.M. SOCIETY FOR PHILOSOPHY AND TECHNOLOGY, *Sutton Parlor North*
8:00 P.M. SOCIETY FOR CLASSICAL REALISM, *Sutton Parlor Center*
8:00 P.M. RADICAL PHILOSOPHY ASSOCIATION and SOCIETY FOR WOMEN IN PHILOSOPHY, *Sutton Parlor South*
8:00 P.M. PHILOSOPHERS FOR SOCIAL RESPONSIBILITY, *Regent Parlor*
8:00 P.M. ASSOCIATION FOR INFORMAL LOGIC AND CRITICAL THINKING *Murray Hill Suite A*

FRIDAY, DECEMBER 28

9:00 A.M.–7:00 P.M. REGISTRATION, *Second Floor Promenade*
9:00 A.M.–6:00 P.M. BOOK DISPLAYS, *Rhinelander Gallery Ctr & South*

Child care arrangements may be made on an individual basis with a commercial firm; see the September or November issue of the APA *Proceedings* for further details.

PLACEMENT INFORMATION

For interviewers, 8:00 A.M.–7:00 P.M., *Bryant Suite*
For candidates, 9:00 A.M.–7:00 P.M., *Morgan Suite*

MORNING

Group Meetings (see Appendix for Programs)
8:30–3:30 COUNCIL FOR PHILOSOPHICAL STUDIES, *Madison Suite*

9:00 NORTH AMERICAN NIETZSCHE SOCIETY, *Beekman Parlor*
9:00 SOCIETY FOR WOMEN IN PHILOSOPHY, *Sutton Parlor North*

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Friday, December 28, Morning

Group Meetings (see Appendix for Programs)

10:00 SOCIETY FOR ASIAN AND COMPARATIVE PHILOSOPHY
Sutton Parlor Center
10:00 SOCIETY FOR THE STUDY OF PROCESS PHILOSOPHIES, *Gibson Suite*
10:00 PHILOSOPHERS FOR SOCIAL RESPONSIBILITY, *Clinton Suite*
10:00 BERTRAND RUSSELL SOCIETY, *Nassau Suite A*
10:00 SOCIETY FOR THE STUDY OF ETHICS AND ANIMALS, *Nassau Suite B*
10:00 AMERICAN SOCIETY FOR VALUE INQUIRY, *Sutton Parlor South*
10:00 SOCIETY FOR THE ADVANCEMENT OF AMERICAN PHILOSOPHY and SOCIETY FOR PHILOSOPHY AND TECHNOLOGY, *Regent Parlor*
10:00 SOCIETY FOR PHILOSOPHY AND PUBLIC AFFAIRS
Murray Hill Suite A
10:00 INTERNATIONAL THOMAS AQUINAS SOCIETY, *Murray Hill Suite B*

NOON

12:00 SØREN KIERKEGAARD SOCIETY, *Beekman Parlor*
12:00 CONFERENCE OF PHILOSOPHICAL SOCIETIES, *Clinton Suite*
12:00 SOCIETY FOR THE PHILOSOPHY OF SEX AND LOVE
Sutton Parlor North
12:00 SOCIETY FOR THE ADVANCEMENT OF AMERICAN PHILOSOPHY
Sutton Parlor Center
12:00 CONCERNED PHILOSOPHERS FOR PEACE, *Sutton Parlor South*
12:00 RADICAL PHILOSOPHY ASSOCIATION, *Regent Parlor*
12:00 SOCIETY FOR THE PHILOSOPHY OF CREATIVITY, *Nassau Suite A*
12:00 INTERNATIONAL SOCIETY FOR NEO-PLATONIC STUDIES, *Nassau Suite B*
12:00 JASPERS SOCIETY OF NORTH AMERICA, *Murray Hill Suite B*

AFTERNOON

2:00–5:00 Session I

A. Symposium: The Philosophy of Susanne K. Langer
Trianon Ballroom
Chair: Ruth Barcan Marcus
Speakers: Arthur C. Danto, Ronald B. de Sousa, Stefan Morawski

B. Symposium: Skepticism, *Mercury Ballroom*
Chair: Michael Williams
Speakers: Margaret D. Wilson and Barry Stroud
Commentator: Robert J. Fogelin

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C. Invited Papers, *Beekman Parlor*
1. 2:00–3:30: On the Concept of the Environment in Evolutionary Biology
Chair: William C. Wimsatt
Speaker: Robert N. Brandon
Commentator: Elliot Sober
2. 3:30–5:00 Contributions and Limitations of Process Philosophy
Chair: Paul Weiss
Speaker: Robert C. Neville
Commentator: David Weissman

D. Special Invited Panel: The Contribution of Applied Ethics to Moral Theory, *Sutton Parlor North*
Chair: Kurt Baier
Panelists: Robert J. Baum, K. Danner Clouser, Kenneth E. Goodpaster, and Richard M. Hare

E. Colloquium: Representationalism in Cognitive Psychology
Sutton Parlor Center
1. *Chair:* Timothy J. Duggan
Dan Lloyd, Mental Representation from the Bottom Up
Commentator: Terry Horgan
2. *Chair:* Walter E. Wright
Radu J. Bogdan, The Paradox of Cognitive Psychology
Commentator: David Sanford
3. *Chair:* Bruce Aune
Peter J. Markie, The Representational-Computational Theory of Mind
Commentator: William Bechtel

F. Colloquium: Realism and Relativism, *Sutton Parlor South*
1. *Chair:* Calvin O. Schrag
J. N. Mohanty, Phenomenology and Relativism
Commentator: Hubert L. Dreyfus
2. *Chair:* John F. Post
David Sedlock, Realism versus Anti-Realism
Commentator: Akeel Bilgrami
3. *Chair:* Carl Ginet
Charles F. Kielkopf, Anti-realism in Kripke's Wittgenstein
Commentator: Paul K. Moser

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Friday, December 28, 2:00–5:00 P.M., Session I

G. Colloquium: Alienation, Mind, and Technology, *Regent Parlor*

1. *Chair:* Robert D'Amico
   Roger S. Gottlieb, Domination and Selfhood
   Commentator: Frank A. Cunningham
2. *Chair:* William McBride
   John Lachs, Alienation in a Mediated World
   Commentator: Mitchell Aboulafia
3. *Chair:* Hans Seigfried
   Michael Heim, The Impact of Word-processing Technology on Human Thought
   Commentator: Janice Moulton

LATE AFTERNOON

Group Meetings (See Appendix for Programs)

5:15 INTERNATIONAL SOCIETY FOR CHINESE PHILOSOPHY, *Beekman Parlor*
5:15 SOCIETY FOR BUSINESS ETHICS, *Sutton Parlor North*
5:15 SOCIETY FOR MEDIEVAL AND RENAISSANCE PHILOSOPHY, *Sutton Parlor Center*
5:15 WORLD PHENOMENOLOGY INSTITUTE, *Sutton Parlor South*
5:15 SOCIETY FOR WOMEN IN PHILOSOPHY, *Regent Parlor*
5:15 CHARLES S. PEIRCE SOCIETY, *Murray Hill Suite A*
5:15 SOCIETY FOR IBERIAN AND LATIN AMERICAN THOUGHT, *Murray Hill Suite B*
5:15 PHILOSOPHIC SOCIETY FOR THE STUDY OF SPORT, *Gibson Suite*
5:15 APA COMMITTEE ON PHILOSOPHY AND MEDICINE, *Nassau Suite A*
5:15 APA COMMITTEE ON TEACHING PHILOSOPHY IN TWO-YEAR COLLEGES, *Clinton Suite*

EVENING

7:15 CONCERNED PHILOSOPHERS FOR PEACE, GANDHI-KING SOCIETY, RADICAL PHILOSOPHY ASSOCIATION, and SOCIETY FOR WOMEN IN PHILOSOPHY, *Nassau Suite A*
7:15 LEIBNIZ SOCIETY OF AMERICA, *Nassau Suite B*
7:15 SOCIETY FOR ANCIENT GREEK PHILOSOPHY, *Beekman Parlor*
7:15 PERSONALISTIC DISCUSSION GROUP, *Gibson Suite*
7:15 NORTH AMERICAN SOCIETY FOR SOCIAL PHILOSOPHY, *Sutton Parlor South*
7:15 SOCIETY FOR THE STUDY OF PROFESSIONAL ETHICS, *Regent Parlor*

8:00 RECEPTION, *Grand Ballroom Suite*

10:00 SOCIETY FOR MEDIEVAL AND RENAISSANCE PHILOSOPHY, *Sutton Parlor Center*

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SATURDAY, DECEMBER 29

9:00 A.M.–5:00 P.M. REGISTRATION, *Second Floor Promenade*
9:00 A.M.–6:00 P.M. BOOK DISPLAYS, *Rhinelander Gallery Ctr & South*

PLACEMENT INFORMATION

For interviewers, 8:00 A.M.–5:00 P.M., *Bryant Suite*
For candidates, 9:00 A.M.–5:00 P.M., *Morgan Suite*

MORNING

9:00–11:00 A.M. Session II

A. Symposium: American Philosophy, *Trianon Ballroom*
Chair: Max H. Fisch
Speaker: John J. McDermott
Commentator: Richard Pruitt

B. Symposium: The Debate about Mental Imagery
Mercury Ballroom
Chair: William G. Lycan
Speaker: Michael Tye
Commentator: Barbara Von Eckardt

C. Invited Paper: Deliberating about the Sea Battle, *Beekman Parlor*
Chair: Jaakko Hintikka
Speaker: Sarah Waterlow Broadie
Commentator: John Cooper

D. Colloquium: Philosophy of Mathematics, *Sutton Parlor North*
1. *Chair:* John P. Burgess
C. Wade Savage, Is Science without Numbers Possible?
Commentator: Hartry Field
2. *Chair:* George Boolos
Clifton McIntosh, Platonism and the Iterative Conception of Set
Commentator: Charles D. Parsons

E. Colloquium: Social Philosophy, *Sutton Parlor Center*
1. *Chair:* George G. Brenkert
Richard J. Arneson, Meaningful Work and Market Socialism
Commentator: David Schweickart
2. *Chair:* Norman Daniels
Joseph M. Grcic, Democratic Capitalism
Commentator: Marlene Fried

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Saturday, December 29, 9:00–11:00 A.M., Session II

F. Colloquium: Ethics, *Sutton Parlor South*
   1. *Chair:* Richard Schacht
      Thomas Bridges, MacIntyre, Nietzsche, and the Unknown Aristotle
      Commentator: Ofelia Schutte
   2. *Chair:* Bernard Baumrin
      Michael J. Zimmerman, Remote Obligation
      Commentator: Earl B. Conee

G. Colloquium: Persons and Purposes, *Regent Parlor*
   1. *Chair:* Owen J. Flanagan
      Marilyn A. Friedman, Autonomy and the Split-level Self
      Commentator: John Martin Fischer
   2. *Chair:* John D. Caputo
      Galen K. Pletcher, The Meaning of the Purposes of Life
      Commentator: Lesley Cohen

H. Special Colloquium arranged by the APA Committee on the Status of Women, *Murray Hill Suite B*
   Topic: Women and the Holocaust: Is There a "Women's Morality"?
   Chair: Kathryn Pyne Addelson
   1. Joan Miriam Ringelheim, Reflections on Women's Experiences of the Holocaust
   2. Barbara Houston, Gilligan and the Idea of a Women's Morality

11:00 RADICAL PHILOSOPHY ASSOCIATION, *Clinton Suite*
11:00 PHILOSOPHERS FOR SOCIAL RESPONSIBILITY, *Gibson Suite*
11:00 APA SUBCOMMITTEE ON NONACADEMIC CAREERS, *Murray Hill Suite A*
11:00 JASPERS SOCIETY OF NORTH AMERICA, *Murray Hill Suite B*
11:30 A.M.–1:30 P.M. BUSINESS MEETING, *Grand Ballroom Suite*

AFTERNOON

2:15–4:15 P.M. Session III

A. Symposium: Patrick Heelan's *Space-perception and the Philosophy of Science, Trianon Ballroom*
   Chair: Ernan McMullin
   Speaker: Lawrence Sklar
   Commentator: Patrick A. Heelan

B. Symposium: Political Philosophy, *Mercury Ballroom*
   Chair: George Sher
   Speaker: Alistair Macleod
   Commentator: Lawrence C. Becker

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C. Invited Paper: Duns Scotus on the Goodness of God
Beekman Parlor
Chair: William Alston
Speaker: Marilyn McCord Adams
Commentator: Eleonore S. Stump

D. Colloquium: Folk Psychology versus Cognitive Science
Sutton Parlor North
1. *Chair:* Robert Schwartz
Morton E. Winston, Whither Folk Psychology?
Commentator: Richard Double
2. *Chair:* Lilly-Marlene Russow
J. Christopher Maloney, Stich's Belief Skit
Commentator: Hilary Kornblith

E. Colloquium: Language, Poetry, and Truth, *Sutton Parlor Ctr*
1. *Chair:* Thomas McCarthy
John McCumber, Habermas and the Language of Poetry
Commentator: Ludwig Nagl
2. *Chair:* Eva Feder Kittay
Lawrence M. Hinman, Metaphors of Truth
Commentator: Merrie Bergmann

F. Colloquium: Modern Philosophy, *Sutton Parlor South*
1. *Chair:* Michael Hooker
Lois Frankel, How Much of "Reason" Does Descartes Need to Validate?
Commentator: Jeffrey Tlumack
2. *Chair:* C. L. Hardin
Joel Friedman, How the Finite Follows from the Infinite in Spinoza
Commentator: Charles Jarrett

G. Colloquium: Ethics, *Regent Parlor*
1. *Chair:* Joshua Halberstam
Edward A. Langerak, Tolerance, Cooperation, and Respect for Wrong Views
Commentator: Ifeanyi Menkiti
2. *Chair:* Roger Wertheimer
Ruth Lucier, Fetal Status and Women's Roles in the Abortion Debate
Commentator: Janet Farrell Smith

4:30 Presidential Address, *Grand Ballroom Suite*
Introduction by Quentin Lauer
Alasdair MacIntyre, Relativism, Power, and Philosophy

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Saturday, December 29, Late Afternoon
Group Meetings (See Appendix for Programs)

6:00 CONFERENCE OF PHILOSOPHICAL SOCIETIES, *Clinton Suite*
6:00 SOCIETY FOR MEDIEVAL & RENAISSANCE PHILOSOPHY, *Regent Parlor*
6:00 GANDHI-KING SOCIETY, *Nassau Suite A*
6:00 SOCIETY FOR WOMEN IN PHILOSOPHY, *Sutton Parlor South*
6:00 SOCIETY FOR IBERIAN & LATIN AMERICAN THOUGHT, *Nassau Suite B*
6:00 AMERICAN SOCIETY FOR VALUE INQUIRY, *Murray Hill Suite B*
6:00 ASSOCIATION FOR THE PHILOSOPHY OF THE UNCONSCIOUS
Murray Hill Suite A
6:00 APA COMMITTEE ON TEACHING PHILOSOPHY IN TWO-YEAR COLLEGES
Gibson Suite
6:00 ASSOCIATION OF PHILOSOPHY JOURNAL EDITORS, *Madison Suite*

EVENING

8:00 INTERNATIONAL SOCIETY FOR CHINESE PHILOSOPHY, *Beekman Parlor*
8:00 SANTAYANA SOCIETY, *Murray Hill Suite A*
8:00 WORLD PHENOMENOLOGY INSTITUTE, *Nassau Suite A*
8:00 RADICAL PHILOSOPHY ASSOCIATION, *Gibson Suite*
8:00 ASSOCIATION FOR INFORMAL LOGIC AND CRITICAL THINKING
Murray Hill Suite B
8:00 SOCIETY FOR WOMEN IN PHILOSOPHY, *Sutton Parlor South*
8:00 AMERICAN ASSOCIATION FOR THE PHILOSOPHIC STUDY OF SOCIETY
Nassau Suite B
8:00 NORTH AMERICAN SOCIETY FOR SOCIAL PHILOSOPHY
Sutton Parlor North
8:00 SOCIETY FOR THE PHILOSOPHICAL STUDY OF MARXISM
Sutton Parlor Center

9:00 RECEPTION, *Grand Ballroom Suite*

SUNDAY, DECEMBER 30

9:00 A.M.–12:00 NOON REGISTRATION, *Second Floor Promenade*
9:00 A.M.–3:00 P.M. BOOK DISPLAYS, *Rhinelander Gallery Ctr & South*

PLACEMENT INFORMATION

For interviewers, 8:00 A.M.–12:00 NOON, *Bryant Suite*
For candidates, 8:00 A.M.–12:00 NOON, *Morgan Suite*

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MORNING

9:00–11:00 A.M. Session IV

A. Symposium: Freedom and Determinism, *Trianon Ballroom*
Chair: Harry Frankfurt
Speaker: Daniel C. Dennett
Commentator: Peter van Inwagen

B. Symposium: Chance, Realism, Quantum Mechanics *Mercury Ballroom*
Chair: Nancy Cartwright
Speaker: Stephen Leeds
Commentator: Allen Stairs

C. Invited Paper: Feminist Reconstructions, *Beekman Parlor*
Chair: Sandra Harding
Speaker: Alison M. Jaggar
Commentator: Nancy Holmstrom

D. Colloquium: Modern Philosophy, *Sutton Parlor North*
1. *Chair:* Daniel Garber
Kenneth P. Winkler, Unperceived Objects and Berkeley's Denial of Blind Agency
of Blind Agency
Commentator: Margaret Atherton
2. *Chair:* Annette Baier
Richard G. Henson, Understanding Butler: An Argument from Sermon II
Commentator: Mark Overvold

E. Colloquium: Political and Legal Philosophy, *Sutton Parlor Ctr*
1. *Chair:* Joshua Cohen
J. Hilary Bogart, Lockean Provisos and State of Nature Theories
Commentator: Arthur Kuflick
2. *Chair:* Theodore M. Benditt
Diana Meyers, A Note on the Problem of Justifying and Differentiating Rights
Commentator: Fredrick Schauer

F. Colloquium: American Philosophy, *Sutton Parlor South*
1. *Chair:* Charlene Haddock Seigfried
Christopher Gowans, From Realism to Foundationalism: in C. I. Lewis (William James Prize Essay)
Commentator: Thelma Lavine
2. *Chair:* Darnell Rucker
Thomas Alexander, The Pepper-Croce Thesis and Dewey's "Idealist" Aesthetics
Commentator: Jonathan Moreno

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Sunday, December 30, 9:00–11:00 A.M., Session IV

G. Special Panel Discussion Arranged by the APA Committee on the Teaching of Philosophy, *Regent Parlor*
Topic: Alternative Approaches to Teaching Introductory Logic
Chair: Dorothy Grover
Panelists: Thomas McKay, James H. Moor, Morton Schagrin

11:30 A.M.–12:30 P.M. BUSINESS MEETING (second session, if needed) *Grand Ballroom Suite*

AFTERNOON

1:30 EXECUTIVE COMMITTEE MEETING, *Madison Suite*

1:00–4:00 Session V

A. Symposium: Philosophy of Law, *Trianon Ballroom*
Chair: Judith Jarvis Thomson
Speakers: David Lyons and Jeffrie G. Murphy
Commentator: Thomas M. Scanlon

B. Symposium: Heidegger/Derrida, *Mercury Ballroom*
Chair: Peter McCormick
Speakers: John Sallis and Edward S. Casey
Commentator: Robert Bernasconi

C. Invited Papers, *Beekman Parlor*
1. 1:00–2:30: Discovering the Forms of Intuition
Chair: Jonathan Bennett
Speaker: Patricia Kitcher
Commentator: Carl J. Posy
2. 2:30–4:00: Moral Realism
Chair: Gilbert Harman
Speaker: Peter Railton
Commentator: Nicholas L. Sturgeon

D. Colloquium: Ancient Philosophy, *Sutton Parlor North*
1. *Chair:* Marcia L. Homiak
Gareth B. Matthews, Gender and Essence in Aristotle
Commentator: Montgomery Furth
2. *Chair:* Michael T. Ferejohn
Richard Mohr, Plato on Time and Eternity
Commentator: Steven Strange
3. *Chair:* Nicholas P. White
Richard D. Parry, Uniqueness and Plato's Third Bed Argument
Commentator: William J. Prior

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E. Colloquium: Aesthetics, *Sutton Parlor Center*
1. *Chair:* Richard Eldridge
Randall R. Dipert, Art, Artifacts, and Regarded Intentionality
Commentator: William Tolhurst
2. *Chair:* Nicholas Wolterstorff
James C. Anderson, Musical Kinds
Commentator: Peter Kivy
3. *Chair:* Dale Jamieson
Joseph Kupfer, Building the Body Politic
Commentator: Stephanie Ross

F. Colloquium: Philosophy of Mind, *Sutton Parlor South*
1. *Chair:* Sydney Shoemaker
Eric Russert Kraemer, Functionalism and Masochism
Commentator: Lynne Rudder Baker
2. *Chair:* John Heil
William S. Robinson, Recursive Behaviorism
Commentator: Joseph Tolliver
3. *Chair:* Hilary Putnam
Jeremiah E. McCarthy, Putnam's Vat World and the Skeptical Method
Commentator: John Koethe

G. Colloquium: Convergence, Frequency, and Probability
Regent Parlor
1. *Chair:* Alexander Rosenberg
W. E. Morris and Robert C. Richardson, Realism, Truth, and Reference
Commentator: LaVerne Shelton
2. *Chair:* Ronald Giere
Deborah G. Mayo, Understanding Frequency-dependent Causation
Commentator: James H. Fetzer
3. *Chair:* Stephen Spielman
Paul Weirich, Expected Utility and Risk
Commentator: Douglas MacLean

PROGRAM PARTICIPANTS
EIGHTY-FIRST ANNUAL MEETING

ABOULAFIA, Mitchell, University of Houston/Clear Lake City I-G
ADAMS, Marilyn McCord, University of California/Los Angeles III-C
ADDELSON, Kathryn Pyne, Smith College, Massachusetts II-H
ALEXANDER, Thomas, Emory University IV-F
ALSTON, William, Syracuse University III-C

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ANDERSON, James C., University of Akron V-E
ARNESON, Richard J., University of California/San Diego II-E
ATHERTON, Margaret, University of Wisconsin/Milwaukee IV-D
AUNE, Bruce, University of Massachusetts/Amherst I-E
BAIER, Annette, University of Pittsburgh IV-E
BAIER, Kurt, University of Pittsburgh I-D
BAKER, Lynne Rudder, Middlebury College, Vermont V-F
BAUM, Robert J., University of Florida I-D
BAUMRIN, Bernard, Lehman College, City University of New York II-F
BECHTEL, William, Georgia State University I-E
BECKER, Lawrence C., Hollins College, Virginia III-B
BENDITT, Theodore M., University of Alabama/Birmingham IV-E
BENNETT, Jonathan, Syracuse University V-C
BERGMANN, Merrie, Wright State University, Ohio III-E
BERNASCONI, Robert, University of Essex V-B
BILGRAMI, Akeel, Columbia University I-F
BOGART, J. Hilary, University of Illinois/Chicago IV-E
BOGDAN, Radu J., Tulane University I-E
BOOLOS, George, Massachusetts Institute of Technology II-D
BRANDON, Robert N., Duke University I-C
BRENKERT, George G., University of Tennessee II-E
BRIDGES, Thomas, Montclair State College, New Jersey II-F
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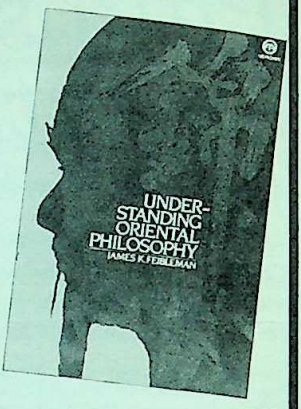

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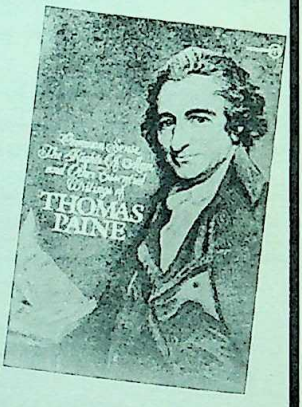

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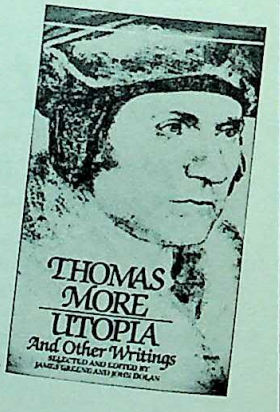

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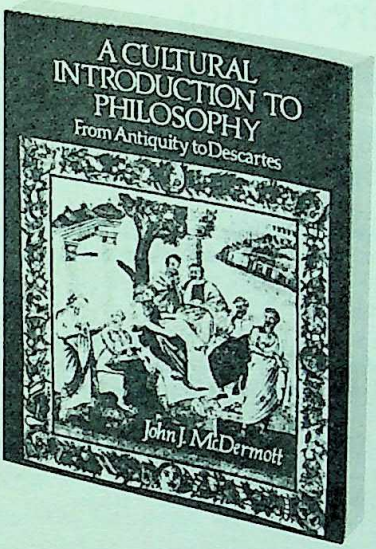

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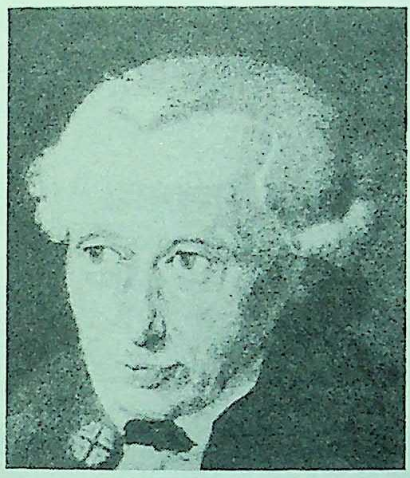

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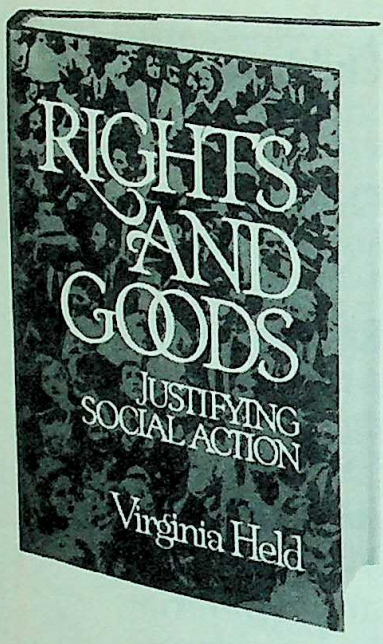

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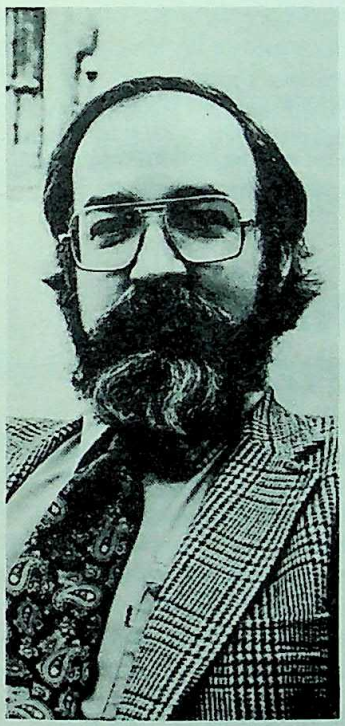

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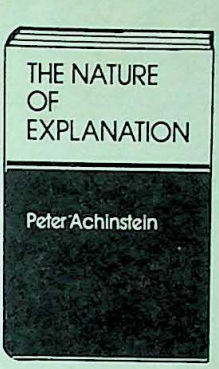

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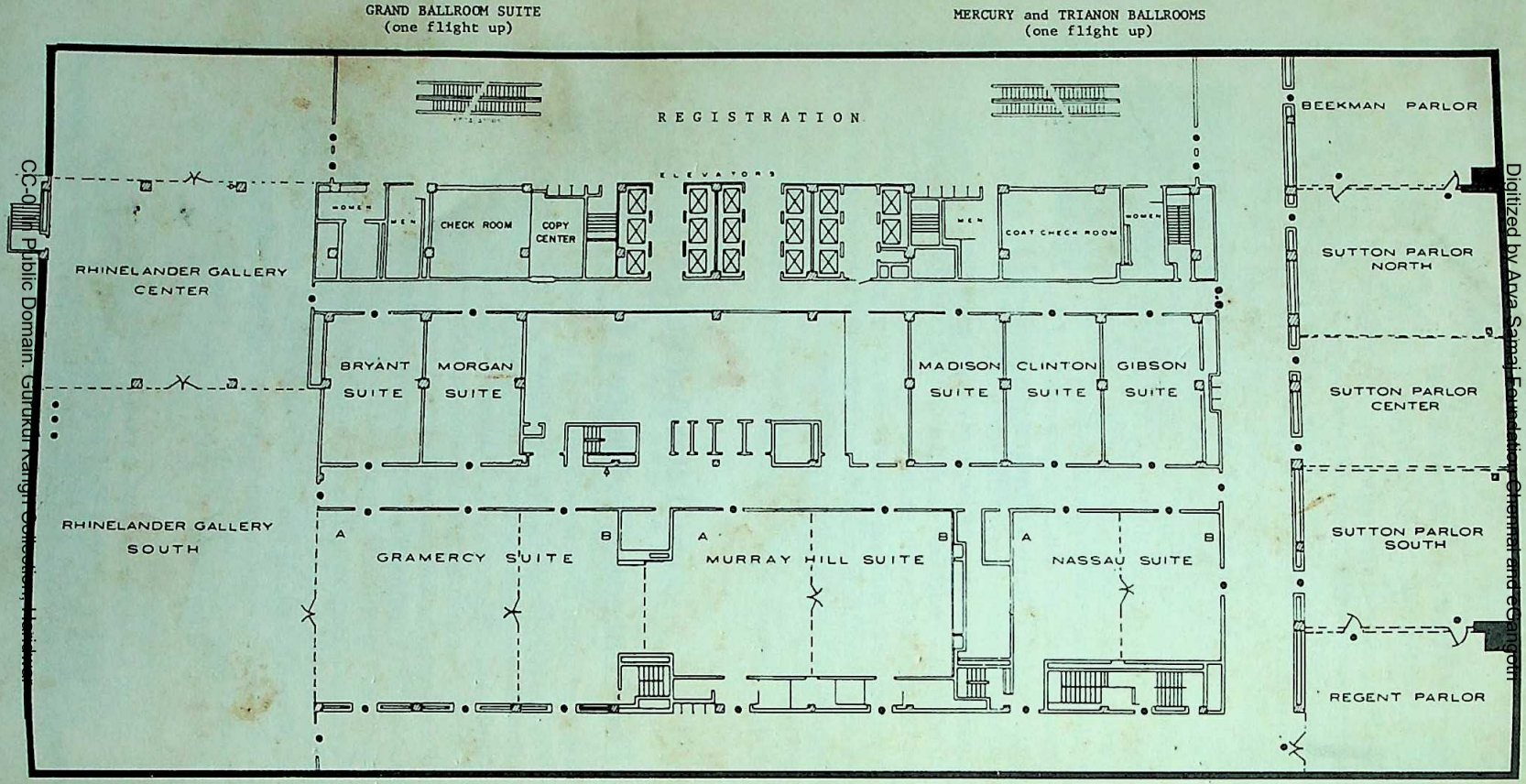

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THE JOURNAL OF PHILOSOPHY

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All communications to the editors, manuscripts, advertisements, subscriptions, and changes of address should be sent to the Managing Editor. Annual subscription price (12 numbers), to individuals, $15.00; to institutions, $20.00; to students and the retired and the unemployed, $12.00; current issues, $2.00; special issues, $3.00. All back volumes and separate issues are available back to 1904. Published monthly as of January 1977; printed by Lancaster Press, Inc., Lancaster, Pennsylvania.

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Fall-Winter 1984-85
$7.50

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ISSN 0022-362X

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THE JOURNAL OF PHILOSOPHY

VOLUME LXXXI, NO. 11, NOVEMBER 1984

MIND AS FEELING; FORM AS PRESENCE; LANGER AS PHILOSOPHER

THE philosophical ascent of feelings, through the long literary career of Susanne Langer, almost presents a political allegory. When we first encounter them, feelings have been dismissed, of course not by her, as the savages of mental life, among whom she moves in the dignifying spirit of an anthropologist rather than the redemptive condescension of the missionary, paralleling those expeditions among the Hopi and the Samoan—or the Mountain Arapesh—then being undertaken by her sisters in ethnography, for whom these people are no more backward westerners than feelings, for Langer, are backward thoughts. Understood in their own terms, the feelings have a cognitive contribution to make, and yield modes of symbolic representation not inferior nor alternative to those of rational thought, but to be set alongside them in the total economy of human life. This was *Philosophy in a New Key*. By *Feeling and Form*, feelings have come a long way from those nondiscursive urges which take rituals and the festal dance as their outward forms: they are taken now as what artworks as a class objectify, and even figure in the definition of art—"The creation of forms symbolic of human feeling"—which is meant to compass the masterpieces of civilizations. But in her final work the feelings have quite taken over: she has so imperialized feeling that even thought is but one of its modes. Thought, perception, imagination, as well as understanding—and of course but only incidentally the emotions—are so many provinces of the empire of feeling. Beginning with the suspicion that there is more to nondiscursive meaning than a scrap thrown to humanists in a gesture of arro-

*To be presented in an APA symposium on the Philosophy of Susanne K. Langer, December 29, 1984. Ronald B. de Sousa and Stefan Morawski will be co-symposiasts; see this JOURNAL, this issue, 647-653 and 654-662, respectively, for their contributions.

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gant generosity by the Logical Positivists, who believed they knew what meaning really was, Langer ends with a claim of almost pre-Socratic audacity and scope.

"I am using the word 'feeling' not in the arbitrarily limited sense of 'pleasure or displeasure', to which psychologists have often restricted it, but on the contrary in its widest possible sense, i.e., to designate anything that may be felt. In this sense it includes both sensation and emotion—the felt responses of our sense organs to the environment, of our proprioceptive mechanisms to internal changes, and of the organism as a whole to its situation as a whole." So she wrote in a prolegomenal essay before undertaking her major work *Mind: An Essay on Feeling*, in which she says "Feeling, in the broad sense of whatever is felt in any way, as sensory stimulus or inward tension, pain, emotion, or intent, is the mark of mentality." It is as though she had followed some philosophical imperative which took her back through the etymological history of the term 'feeling' to the Greek word '*φαλλειν*'—"to pluck, twitch the harp, play a stringed instrument"—and thence to the substantive *φαλτήριον*—"a stringed instrument"—to rediscover the marvelous metaphor of the soul as a lyre, from which harmonies are evoked by the impingements of the world, and philosophy in a new *key* becomes abruptly literal. Like Simmias in the *Phaedo*, before he is bamboozled by a bad argument of Socrates, she sees the harmonies not as something apart with a life of their own, but as a phase of the instrument. Feeling, hence mind, construed as a physiological state, is something the study of which "leads down into biological structure and process until its estimation becomes (for the time) impossible, and upward to the purely human sphere known as 'culture'." A resolute materialist and a psychological realist, Langer regards what today would be called the categories of folk-psychology as at once ultimate and neurophysiological. As I find this position uniquely correct, I shall in this paper be less concerned with the assimilation of feeling to nervous matter than with the assimilation of mind to feeling. That assimilation too may be correct, but it has to be defended by structures of argument different from those which may finally vindicate a retentive materialism.

Scrutinizing *Philosophy in a New Key* in the retrospective light of *Mind: An Essay on Feeling*, one may discern in it anticipations of the expanded view of feeling, which, in a curious way, Langer owes to the Positivists. For it was their polemical stance that a vast array of linguistic expressions, ranging from verbal grimaces through lyrical poetry to *Die Welt als Wille und Vorstellung*—or in

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any case *Sein und Zeit*—are meaningless in respect to the favored way of being meaningful, namely verifiable through sense experience, but may in compensation claim a secondary mode of meaningfulness that the Positivists disdained to differentiate: invariably they express their users' feelings and have emotive meanings. The picture of the mind the Positivists endorsed, as an island of discursivity in a sea of affect, is rather like the famous *New Yorker* cover which divides the world into Manhattan and then simply everywhere else. Carnap's bisection has about that degree of sensitivity to nuance and extenuation as the partition of India: "Many linguistic utterances are analogous to laughing, in that they have only an expressive function, no representative function. Examples are cries like 'Oh,Oh' or on a higher level, lyrical verse. Metaphysical propositions are . . . like laughing, lyrics, and music, expressive." If all this stammering vastness of leftover mind is feeling (and we must adjoin moral discourse), Langer has practically been handed her thesis. All that remains is to colonize thought itself, already half achieved in the suggestion that what she terms "discursive forms" *express* thoughts as presentational forms express feelings, which have their proper forms much as thought has its proper forms.

"The thought is the significant proposition" is Thesis 4 of the *Tractatus*, a work admiringly cited in *Philosophy in a New Key*, and though "thought" may, in post-Fregian ontology, more or less be "meaning," which ingresses propositions from Frege's paraplatonic Third Realm, it is also possible to interpret a thought as a mental state in propositional form. For Aristotle, speech stands to thought as writing stands to speech, as though the same propositional form might occur in three distinct media. Spoken or written propositions might then express mental ones (= thoughts)—or *objectify* them—in just the same way, though the forms will be nonpropositional, that nondiscursive forms objectify nondiscursive mental states (= feelings). Langer's categories of feeling and form are subdued versions of Schopenhauer's categories of will and representation, and when in her philosophy works of art are feelings objectified, the clear implication is that art is the outward form of feeling as discourse is the outward form of thought (for Schopenhauer, representation is "the will become object"). So it is clear she will want to distinguish symbols from symptoms, since humid palms, shallow breathing, dilated pupils more evince than express feelings—and for the matter even thoughts may have visceral concomitants: Darwin said "I remember well the time when the thought of the eye made me cold all over." A line can thus be drawn between 'Oh, Oh' as a kind of disciplined grimace and *Ode to a Nightingale*

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as the outward symbol of some feeling about nightingales (or about what nightingales themselves symbolize) it succeeds in expressing: feelings have forms perhaps only when they have contents or what today we might call "intentionality." The question then is what evidence we have that nondiscursive feelings have forms which artworks express just as thoughts have forms which discursive forms express, granting that both modes of expression may be classed as objectifications.

Some evidence, or even a strong confirmation of Langer's general thesis may be allowed if we grant her claim that sense perception is a case of feeling. "All sensitivity bears the stamp of mentality," she writes, "seeing is itself a process of formulation." So forms that show what we see might then be objectifications of vision, and a bridgehead is established. On the other hand, despite the support Gestalt psychology provides for sensing having form, it somehow seems a kind of cheating to pass from the mentalization of sensation to the further assimilation of sensation to feeling. A colleague once told me he especially admired Whitehead for leaving room in his metaphysics for feelings. A good while later, thinking it might be useful to see what Whitehead had to say, I found myself struggling with the concept of *prehension,* which feelings then exemplified. "A feeling," Whitehead wrote, "is the appropriation of some elements in the universe to be components in the real internal constitution of the subject." But when he steps outside the fiercely fortified language of his system he says: "As a simple example of this description of a feeling, consider the audition of a sound. In order to avoid unnecessary complications, let the sound be one definite note." Well, if this is leaving room for feelings, what philosopher down the ages, with the possible exception of Parmenides, has not? He has merely left room for perception. At a certain point in the history of modern art, when painters were producing images so discongruent with what visual experience would lead us to expect if their aims were to present reality as seen, it was a genuinely illuminating explanation that they were more concerned to express their feelings about what they saw than merely to replicate it. But if seeing itself is feeling, one is also expressing one's feeling in painting what replicates the visual array. Something has been blurred in this expansion, which must surely also be what in his romantic heart moved my colleague to praise Whitehead for leaving feelings in!

Nor does Langer's discussion of the famous distinction between two orders of symbolic form particularly help, in part because her example of a presentational form is a picture of what we see ("the

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most familiar sort of nondiscursive symbol"), but more subtly because she at the time endorsed the curious pictorial semantics of Wittgenstein as her analysis of discursive forms. So it is as though we were contrasting two orders of picture, since, in her words which echo Wittgenstein's "The proposition is a picture of reality," "Language (is) our most faithful and indispensable picture of human experience, of the world and its events, of thought and life, etc." Wittgenstein was driven to the extreme of pictorializing language in order to connect it to the world, since the projective relationship supposedly exemplified in pictorial representation must have struck him as clearly understood or at least perspicuous. But in fact only in the case of quotations is a projective relationship between sentence and subject plausible, and it is not the projective relationship which explains how pictures in fact present.

What Langer wants, I believe, is something closer to Plato's division of poetic representation into *mimesis* and *diegysis,* a distinction which incidentally arises within language itself. In dramatic representation (Plato's example), the actor's words are the character's words, not a description of what they were. In this sense we have no idea of what *commedia dell' arte* was like, since we lack the words: we just have knowledge of what they were to convey: jealousy, suspicion, passion, innocence, and contempt. The *avant-garde* artist Robert Morris once said of his blank works "They're not trying to represent anything. They're presenting an object." But this is exactly mimetic presentation in Plato's sense: there has always been a suspected power in pictures and effigies, which explains their becoming objects of worship, since the icon might contain, as a bucket contains blood, the saint herself, rather than a mere projection of her features and emblemata. So we are dealing here with something more fundamental in cognitive address than anything that can be captured in the analog/digital distinction that Langer's argument and examples make it appear she is suggesting. And now, if there are forms that present feelings, as distinct from what we merely sense, then she will have made her point. Let me conclude by proposing how.

Proust's readers are given a body of propositional knowledge regarding *la maison de Tante Léonie* in Combray, which pilgrims to the actual site in Illiers will confirm. They will also realize, being pilgrims, that it is not at all what they have been led to expect: what the house is actually like cannot have been predicted from the descriptions; it is cramped and dark, with a garden far too tiny. But at a certain moment it dawns that in the course of describing the house Proust has presented what it felt like to have lived there at a

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certain age. By a miracle of discursion he has not described but transmitted his feelings. The same can take place with pictures. The art historian Howard Hibbard told me once of driving up and down the road to Benevento in search of a Roman tower he had seen in (*in—nota bene*) an engraving of Piranesi and had been assured still existed. It was invisible, because he had expected something monumental rather than the stump of masonry someone finally indicated. The *projection* was exact, but what the engraving showed was the tower as felt by a man with a sense for Roman grandeur. But what is objectified in both these representations, verbal and pictorial, and as invisible to the senses as the feelings toward Combray transsubstantial in tea-steeped madeleine, is that kind of feeling Susanne Langer identifies finally as mind. Whether art alone objectifies feeling or whether it is objectified in everything we do, she is perfectly correct in saying that the subjectivity of felt experience, the lived presence of the world, is the central fact of mental life.

It is not a fact psychologists have countenanced, let alone approached; and it has been made a problem for philosophers only recently, I believe, through the thought of Thomas Nagel. We have and may add to a body of propositional knowledge regarding the stimuli bats respond to, but there must be something that would answer the question of what it's like to *be* a bat which we know this knowledge will never give us. But we also know that propositional knowledge will never reveal what it is like to be, well, Susanne Langer or, closer still in the present case, Arthur Danto. I know a great deal about myself nobody else knows, or so I hope; but I could tell you all this at boring length without transmitting what it's like to be me, which I am not sure I know, never having been anyone else. The truth, I think, is that, however discursive, we are largely invisible to ourselves, save perhaps obliquely through the manner in which others may *present* themselves through their words and actions: so invisible that there is a natural philosophical temptation to construe the mind as a pure transparency, a mere openness to the world, rather than the densely knotted tissue of feeling of which we are enfabricated.

It may be that, through the cognitive asymmetries that discursive and presentational knowledge define, a way is open to fit thought into the substance of feeling. We know that people think certain things, can define the content of their thought, without having it in our power to think those things ourselves. What is it like to think the world flat, that I look irresistible in my Poiret frocks, that the Reverend Jim Jones would have saved me through his love, that

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animals have no feelings or that flowers do—or that punk is where it's at? For me to think any of these things I would have to be a different person with a different mind, since a lot would have to be different among my other thoughts for any of these to fit effortlessly among them—and so I would live in a very different world since my world is specified by my thoughts. But then the gaps between me and those who think differently than I—which is to say everyone, and not simply those segregated by the differences in generation, sex, nationality, sect, and even race—must define the real boundaries of the self. The plane of contact between the self and the world, inestimably more complex in its curvatures than those of its sub-areas in which sense and stimulus make contact, is the reality of mind as Langer has sought to grasp it philosophically. Merely to have recognized its character, let alone carried its analysis so systematically far, has been to make as immense and transformative a contribution to philosophy as I can imagine.

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TELEOLOGY AND THE GREAT SHIFT*

> [A] veritable gulf . . . divides human from animal mentality, in a perfectly continuous course of development of life on earth. . .
>
> Teleology is peculiar to vital process, and since it appears in psychical phase as the pattern of aims and voluntary acts, . . . it is a central theme in the study of mind; but it has had the least successful philosophical treatment of all our major problems.†

IN tackling the "Great Shift" from animal behavior to human mentality, Susanne Langer has stressed four principles:

1. Look for a unifying *biological* perspective in which the emergence of mind can be seen naturalistically as a "continuous course of development."

*To be presented in an APA symposium on the Philosophy of Susanne K. Langer, December 29, 1984. Arthur C. Danto and Stefan Morawski will be co-symposiasts; see this JOURNAL, this issue, 641-647 and 654-662, respectively, for their contributions.

†Susanne K. Langer, *Mind: An Essay on Human Feeling*, 3 vols. (Baltimore: Johns Hopkins, 1967, 1971, 1982), vol. I, p. xvi and p. 220. Subsequent references to Langer will be to that work and will appear in the text either by chapter or by volume and page number.

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2. View as fundamental the notion of an *act*, in a sense that will partly explain, but does not presuppose that of full agency.
3. Take account of the crucial importance of the process of *individuation*.
4. Seek the essential differentia of human mentality in the production and use of *symbols*.

In this paper, I propose to sketch how these principles can provide a perspective on some current ideas about the role of teleology in the Great Shift to human mentality.

Langer has decried the anthropomorphism of much ethological talk about instinct (see ch. 15). She holds that "all animal behavior is instinctive, arising from organic sources as impulse seeking expression in motor action" (II 45) The notion of "impulse" calls for explication in teleological terms. But for that we need a conception of teleology that is neither simply reductionist nor based on a confused attribution of conceptual capacities to lower animals (II 16). Such a notion now exists, and can be roughly sketched as follows:

Teleological laws are a special case of causal laws, distinguished by additional structure in their antecedent. In the general form of a law (1):

$$(1) \qquad (x)(Ax \rightarrow Bx)$$

where the arrow is a suitably interpreted connective, Ax can be expanded as in (2):

$$(2) \qquad (x)(Bx \rightarrow (\text{probably})Gx)$$

The resulting schema is (3):

$$(3) \qquad (x)((Bx \rightarrow Gx) \rightarrow Bx)$$

If (3) is true, then we can say that the *function* of B is to promote G. Informally, B has that function if its existence can be explained in terms of its tendency to bring about G as a consequence.[1] This analysis provides a genuine analysis of the notion of goal. [G in (3)

[1] My formulations ignore many necessary refinements. These are sufficiently detailed in recent literature, on which my broadly sketched account draws freely. See especially Charles Taylor, *The Explanation of Behaviour* (New York: Humanities, 1964); Andrew Woodfield, *Teleology* (New York: Cambridge, 1976); Jonathan Bennett, *Linguistic Behaviour* (New York: Cambridge, 1976), ch. II; Larry Wright, "Functions", *Philosophical Review*, LXXXII, 2 (April 1973): 139–168; George A. Miller, Eugene Galanter, and Karl H. Pribram, *Plans and the Structure of Behavior* (New York: Holt, 1960); William G. Lycan, "Form, Function, and Feel," this JOURNAL, LXXVIII, 1 (January 1981): 24–50. Reasons of space preclude my noting points of contact and difference. Nor can I discuss criticisms, which in my view the account survives. But see, e.g., Christopher Boorse, "Wright on Functions," *Philosophical Review*, LXXV, 1 (January 1976): 70–86, and Robert Cummins, "Functional Analysis," this JOURNAL, LXXII, 20 (Nov. 20, 1975): 741–760.

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is a "goal" in virtue of its place in the schema.] It applies equally to everything for which we intuitively think teleological or functional explanation applies: artefacts, instinctual behavior, and intentional acts. The differences among those classes must be sought in the type of explanation that must be given for the fact that the teleological law holds in each case.

To show this, I begin by noting a defect of the formulation just given. Any causal law $(A \rightarrow B)$ can be trivially given teleological form (4):

$$(4) \qquad (x)((Bx \rightarrow I(A \rightarrow B)) \rightarrow Bx)$$

("Bx happens whenever it results in the instantiation of the law.") We need to add the qualification that ordinary causality is the default assumption: the label 'teleological' applies only to what cannot be reformulated in *non*teleological form. The additional structure in (2) must be *ineliminable*. But when is this the case?

Ernest Nagel has suggested that reduction of a teleological law can be effected by finding a categorical condition materially equivalent to the conditional forming the antecedent.[2] Thus, in an animal tropism, the condition *is in a state in which turning right is generally conducive to increased light* might be replaced by *is caused by light gradients affecting its sensors to orient itself to the right*. The trick is to find such a mechanism. Charles Taylor has speculated that, in some cases, as a matter of empirical fact, none would be found. These would be cases of basic (i.e., irreducible) teleology. Certainly this can't be ruled out a priori. But perhaps it should be ruled out on methodological grounds. For even if there are such cases, that fact could never be established. The most we could know at any particular time is that we haven't yet found the right material equivalent. What, in the light of past progress in finding mechanisms to explain apparently "insightful" or teleological behavior, could justify giving up the search? Brute teleology would represent a cul-de-sac of science.

Nevertheless, Nagel's strategy may not always be the best. The categorical mechanism may be epistemically inaccessible. In such cases we might be able to establish the truth of the teleological formula beyond reasonable doubt, but the mechanism would remain a matter of speculation. More important, finding the mechanism may not give us the explanation we seek. What we need to understand is how it came about that some mechanism goes into action *just when it is likely to have a certain result*. That, whatever the mechanism, is what the teleological law asserts.

[2] *The Structure of Science* (New York: Harcourt, Brace, and World, 1961), ch. 12.

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Two sorts of explanations are available: in terms of *natural selection*, or of *representation and planning*. These are, respectively, associated with the two types of prima facie teleological events: biologically adaptive characteristics, and those acts which result from mentality. The fit is not perfect, of course. We usually think of learning as involving mentality. And at least one kind of learning is simply the result of selection over a population of acts rather than of organisms. It is crucial to a proper understanding of the Great Shift to parcel out those cases which involve true mentality from those which are directly explicable on the selectionist model.

Sociobiologists have recently come under much fire, precisely for applying (gene-)selectionist explanations to behavior subject to human motivation. The charge is not sound in principle. It confounds two quite independent issues. One is whether the immediate or *proximate* causes of the behavior in question involve mental representation; the other is whether the behavior is *ultimately* explicable in terms of gene selection. The answer might be Yes to both; for nature selects for outcomes, not proximate causes. The cooperative behavior of humans and the "cooperative" behavior of ants, for example, belong to opposite ends of the Great Shift. At the level of proximate cause, they must be completely different. But from the evolutionary point of view they might be due to similar selective pressures.

So what is the crucial difference? Following Langer's lead, we can hope to make progress if we can find intermediate forms. Can we find elaborate acts or act-sequences that owe their teleological appearance to the fact that they involve a primitive form of representation?

Consider Langer's characterization of instinctive acts: "The beginning and the end of the act are its essential elements—impulse and consummation—spanned by a single arc of nervous tension" (II 68). "Impulse" is not mere cause, nor is "nervous tension" to be taken literally as a quantity of energy to be discharged. The terms are to be taken teleologically. Whether the act is learned or innate, it must involve feedback. And the mechanism involved in the testing or evaluation of feedback must already have some characteristics in common with representation. It must make discriminations, in accordance with which activity continues, is modified, or stops.

This is sufficient to give the structure of the act its teleological form. It also involves two features traditionally associated with intentionality:[3]

[3] The ideas in this paragraph derive closely from an important unpublished paper by Anne MacKenzie on "The Intentionality of Desire," though MacKenzie may not approve of the use to which I put them.

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1. *Intentional Inexistence:* the condition "represented" in the test mechanism may never actually occur. This is the functional analogue of failure of reference in belief and of failure of satisfaction in desire.
2. *Generality:* This is to be taken in two senses:
   a. In contrast to *specificity*. Animals can be fooled by relatively coarse decoys. It is costly to refine perception and guard against unlikely mistakes. Selection finds an equilibrium at a degree of specificity adequate for practical purposes. This goes for learning as well as evolution. But the point is not merely an economic one. It is also logical. This brings in the second sense:
   b. In contrast to *particularity*. However finely or coarsely it is specified, the "object" represented will always remain a *general type*, never a particular.

These features define what I shall call *quasi-intentionality* (Anne MacKenzie aptly calls it "nonmental intentionality"). They are shared by full-fledged intentionality, which Roderick Chisholm argued was coextensive with the mental.[4] Intentional inexistence was one of his criteria. But generality is also a feature of full-fledged intentionality, as we can see from the contrast between causality and explanation. Actual causal relations are always between particulars. They are neither mental nor intentional. But not every feature of particular events is relevant to the causal explanations in which they figure. Explanations are intentional, in that they require only a degree of specificity.[5]

Quasi-intentionality cannot escape either form of generality. However specific the testing mechanism in the feedback loop, any particular that fits the "representation" involved will have properties left unspecified. Thus far, quasi-intentionality is like the real thing. But full-fledged intentionality *can* pinpoint a particular individual (though it need not). It is necessarily general in sense a, but not in sense b. We have found here, I suggest, a pivotal feature of the Great Shift.

An objection must be faced. I have earlier admitted as a matter of methodological principle that no teleology can be considered irreducibly basic. My examples of quasi-intentionality all involve mechanisms. Talk of "representations," in such cases, can only be honorific. Teleology is understood only if it is reduced, i.e., if a mechanism can be found corresponding to the organism's capacity to detect the "test condition," But why then speak of teleology at

[4] "Sentences about Believing," *Aristotelian Society Proceedings*, LV (1955/56): 125–148.

[5] See Donald Davidson, "Causal Relations," this JOURNAL, LXIV, 21 (Nov. 9, 1967): 691–703.

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all? Why not simply say that some mechanisms are usefully viewed as teleological for heuristic purposes, just as long as they are not fully understood?

To answer this, we can draw inspiration from Langer's stress on the importance of *individuation* (ch. 9). Langer points out that individuation in organisms is a matter of degree:

> The activities that compose a particular life . . . are so many and so variable that even in a single organism individuation may go to different lengths in separate directions (I 337).

This draws attention to the fact that we can make sense of teleology only if we can single out a process or (in Langer's preferred term) an *act*, caused *as a unit type* by either selection or intention. This can be illustrated without reference to the issue of mentality. A recent article[6] develops the idea that we can speak irreducibly of the immune system's making *errors*. One example is auto-immune response, triggered not by some genuinely "foreign" substance but by "misrecognition of self." Even if we can always give a merely chemical account of the response and its causes, this may result in the loss of an epistemologically indispensable level of explanatory information. In the course of any particular organism's life span, a selection process has led to the establishment of the immune response in question, But it is only if we think of it as a response *of that particular organism as such* that we can identify some of the effects of the mechanism as "normal" and others as "errors." And this, in turn, requires that we take seriously as a natural fact the individuation of the organism affected. Various epistemologically useful concepts, "individual"; "response"; its "causes and effects"; "teleological law", are tied together into a relatively autonomous level of explanation (though this is not, of course, a "basic" level of explanation in Taylor's sense).

Let us now return to the quest for the difference between the instinctual grade and the full-fledged mental grade of intentionality. A good place to start is with those of Chisholm's original criteria which do not figure in my definition of quasi-intentionality.

One such criterion is *failure of substitutivity of co-referential terms*. Here, as in the case of the definition of teleology, we must note that we can always extend the language of full-fledged intentionality to mere tropisms. But we don't have to; and good method demands that we do so only if we must. Suppose a hungry frog is in the presence of a freshly killed fly. The fly is food. It is present to the frog. Yet the frog does not eat it. We might describe this sit-

[6] Edwin Levy and Mohan Matthen, "Teleology, Error, and the Human Immune System," this JOURNAL, LXXXI, 7 (July 1984): 351–372. See esp. 355, 363.

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uation as one in which the frog has failed of some teleological law: *hungry frogs will eat available food.* It desires food; it does not desire that motionless speck. Yet that motionless speck is food. So do we not have here failure of substitutivity? No. When described at the right level of neurological fact, the frog was not in the presence of the right stimulus. Epistemic hypotheses are unnecessary: frog's eyes simply do not detect motionless specks.[7]

In any case there is an ambiguity in the notion of substitutivity of co-referential terms. The substitutivity failure rejected in the case of the frog concerned only representations of materially equivalent *types*. But what typically fails, in fully intentional contexts, is substitution of singular terms referring to material particulars. I conclude that the capacity for singular reference constitutes one necessary condition of full-fledged (mental) intentionality.

The concept of reference does not figure importantly in Langer's account of "symbolism," to which she looks for the key to her Great Shift. But the preceding discussion suggests that the capacity for singular reference is of crucial importance. Singular reference is presupposed by full-fledged human memory and intentional action. It requires a conceptual scheme capable of distinguishing between *qualitatively indiscriminable* individuals. This in turn means that the organism in question must have a *biography*. For no expression can genuinely refer to a particular, if there has been no moment at which that reference was fixed for some particular user. For this reason, no innate idea could ever actually refer, except in the Pickwickian sense in which a chiromantist's client might exclaim: "A tall dark man! That must be George!" Reference can, of course, be inherited second-hand from other members of a community of users. So it commonly presupposes the kind of social matrix which many have on independent grounds thought crucial to the human difference.

These features, I submit, make singular reference a promising criterion of the passage from quasi-intentionality to full-fledged mentality. The analysis of reference itself is no easy task. Certainly I have none to offer. But though it poses a hard problem, we can hope that it covers a narrower gap than the general difference between instinctive and mentally motivated behavior. In this way it represents a promising implementation of Langer's strategy of sidling across the Great Shift in small steps.

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[7] See J. Y. Lettvin, H. R. Maturana, W. S. McCulloch, and W. H. Pitts, "What the Frog's Eye Tells the Frog's Brain," in Warren S. McCulloch, *Embodiments of Mind* (Cambridge, Mass.: MIT Press, 1965).

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ART AS SEMBLANCE*

MY purpose in this essay is to contribute in some way to a firmer understanding of the pivotal role exerted by Suzanne Langer in the fifties. I cannot, in this brief space, explore the broad context of aesthetic thought as it developed and crystallized in Europe and the U.S., before *Feeling and Form* was published. Instead, I will focus on the problem of *semblance,* and on the original solution of it which Langer provided. It will, I hope, be of some service to my readers if I delineate at the outset where I am heading. Granting that this symposium is only the beginning of a renewed grappling with the significance of Langer's so-called "presentational symbolism," I would like to assess her style of thinking and her distinctive approach to the philosophy of art. To this end, I shall attempt first to highlight her closest philosophical pedigree, concentrating on Ernst Cassirer, to whom she often acknowledged her intellectual debt. I shall ask, in turn, what are the strengths and weaknesses of Langer's aesthetics, focusing on the idea of semblance which is central to her project. Then, I shall confront Langer's standpoint with the late views of Theodor Adorno and with Arthur Danto's recent analysis, in order to elicit the attraction that Langer's standpoint continues to hold.

In the introductions to *Feeling and Form* and also to the anthology, *Reflections on Art,* Langer cites a number of sources upon which she draws. The list includes such philosophers as Kant, Bergson, Croce, the aestheticians E. Bullough, K. Lange, D. Prall, R. Collingwood, the theories of art of F. Schiller, A. Hildebrand, C. Bell, R. Fry, as well as Freud's theory of the unconscious, and selected essays by O. Baensch, C. Morgan, G. Mehlis. Though the names belong to different strands of philosophical and aesthetic thought, they contribute, at least from Langer's perspective, to one current of thought, and provide the indispensable clues to sound theory. Langer assimilates from her predecessors everything that seems worthy to her of revalued continuation and further cultivation. Even some fleeting hints are credited by her with great attention, if they fit Langer's own pattern of thought. In this sense, though she tries to be as flexible as possible, there remains a kind of orthodoxy in such an attitude. The selected thinkers are treated as members of the same intellectual family, seeking the same distilled philosophical truth.

*To be presented in an APA symposium on the Philosophy of Susanne K. Langer, December 28, 1984. Arthur C. Danto and Ronald B. de Sousa will be co-symposiasts; for their contributions see this JOURNAL, this issue, 641–647 and 647–653, respectively.

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This carefully and precisely carried out analysis and criticism of what is to be found in the philosophic and aesthetic heritage in order to facilitate and necessitate the best selection brings us directly to Cassirer. For what Langer furnishes us with is the consciously appropriated history of ideas done from a given outlook, a research strategy which does not draw any sharp distinction between this history of philosophy and philosophy itself. Langer is thus no simple historian, but remains a philosopher also when she affords us an enlightened account of the relevant past from which she draws her stimulation. This approach stems from Cassirer, and is characteristic of the whole Marburg school. Hermann Cohen and Paul Natorp, as we know, tackled classical Greek thought not for its historical value, but rather to show what genuine philosophy should be like. Langer's stance, i.e., her reiterated polemics with empiricism, naturalism, and with neo-positivist philosophers of science, her insistent cutting herself off from metaphysics and psychology—all these bear on the Marburg version of neo-Kantianism which characterizes Cassirer's work.

Cassirer transcended the boundaries of his Marburg teachers, taking culture and the humanities as the domain of his analysis. He reformulated the fundamental question that Kant articulated with respect to the strict sciences, posing it instead in the form: How is knowledge about spiritual life (*das Geistige*) possible? How can legitimate knowledge be attained when we inquire into language, myth, religion, art, philosophy, as well as when we inquire into the sciences? As we know, the monumental work in which Cassirer examined myth, language, and science meticulously from the same methodological point of view is his three-volume *Philosophy of Symbolic Forms*. The over-all method of investigation implies that culture is an organic whole, the core of which is the same cognitive function, namely, the symbolic form materialized in given vehicles. For Langer, who inherits and elaborates Cassirer's theory in her own way, art is seen as a symbolic form of a special kind, an objectified expression of the felt life, but yet one which retains its conceptual character. Its concepts are nondiscursive, its symbolics is different from that of the natural sciences or mathematics, its logic consists of articulate forms which are the opposite of language, for they possess no vocabulary, no singled out, discrete units, no particularized reference to the fragments (objects) of the outer reality. The cognitive value of art lies rather in its presenting the structure of inward life, the idea of the imagination of feeling. Art then is the symbolic form of impersonalized subjectivity mediated through its material vehicles and its specialized devices or techniques.

Langer's conception, it may be said, suffers from the same draw-

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backs as does Cassirer's. It invites a confusion with respect to philosophy, science, and art, if they were to be ranked according to the logical-historical order that the theory proposes. Why should science be regarded as a higher degree of constructing a *Weltbild* than the others? And, if one grants Cassirer's (and Langer's) view that philosophy is nothing but the highest possible understanding of the human participation in and creation of culture, that it does not establish any irrefutable truths, but rather gives rise to insights, that it is estimated not by its scientific truth, but by its understanding of the forms of knowledge—even granting all this, why should it turn out that the most adequate philosophical approach should be that grounded in Marburg neo-Kantianism? What justifies the typology of symbolic forms? Why is the distinction between the criteria of *quality* and *mode* left without any reasons given to justify it; and why are we not told whether these criteria are obligatory or only facultative? The arbitrariness of the solution is clearly seen in *Feeling and Form*: all other philosophical models are criticized for not being consonant with the given view of the a priori principles of how the human mind works; the virtual realm of art is divided into compartments based on the schema of *quality* (the fine arts, music, dance) and *mode* (literature, drama); film is interpreted as the domain of the perpetual *Now* and presented as the counterpart of Cassirer's characterization of myth (though without any solid arguments); the novel is understood as that form which gives symbolic expression to the past, whereas drama gives expression to human destiny. Granted, Langer's account is full of flashes of invention and insight. But it could hardly be said to capture the essence of the forms of the plastic, the literary, the dramatic, the comic, the tragic, the lyrical, or the epic, etc.

Nonetheless, these shortcomings do not alter the fundamental fact that it was Langer who revived and reargued the Kantian-Schillerian idea of semblance and who filled the lacuna within the framework of neo-Kantian philosophy; for there, the aesthetic domain had until then only been touched on in Cassirer's *oeuvre*. Thus, it took almost a half century for the Marburg school to come up with a significant aesthetic conception. In the face of the then-reigning disputes over whether mimesis is the chief category of art, and of the debates between linguistic analysts and Crocean intuitionists, in aesthetics, Langer's *Feeling and Form* introduced a glorious "return to Kant" by way of the philosophy of culture. Langer's contribution was of enormous weight. And incidentally, it had an important effect on my own thought as well.

I remember my first reading of *Feeling and Form*. It must have

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been in 1954, or in any case, several months after Stalin's death. The atmosphere in my country was mostly unsympathetic for a Kantian revival, and I myself was deeply engaged in adapting my philosophical background (The Warsaw-Lwow school) to Marxism. The Soviet literature on the subject of aesthetics (the apprentice of which I attempted eagerly to be) led me nowhere. It happened to be mostly a waste of time and mental energy. I turned back to Marx and discovered—(This may strike you as sophistry, but is nevertheless true)—that he was a kind of Kantian in the sphere of aesthetics. Naturally, Marx posed different questions than Kant did and his manner of thinking about art's genesis and function had to be (because of his world view) completely divergent from Kant's basic ideas. However, with respect to the question of what makes art distinct from other forms of human (social) consciousness, he was simply the heir of German idealism, and primarily of Kant's *Critique of Judgment*. True, he does not refer directly to the category of *Schein*, of semblance, but he respects it tacitly. He explicitly adopts the idea of "the free play of spiritual energies," when he characterizes the nature of aesthetic experience.[1]

Digging at that time into Langer's work, I gratefully discovered her paramount category of *virtuality*, i.e. the self-sufficient microcosm of any artwork that does not appeal to mere sensuous response or merely offer occasion for the expression of the artist's feelings or simply reproduce the outer reality, Zeuxis-fashion, to deceive the birds with painted grapes. Rather, as Langer made clear, this virtual object, this artwork, elicits something different, which must be rendered as *strangeness, otherness, separateness*, despite the obvious relationship between the artwork and human life, its external surroundings and its inner realm. It was clear to me at the time that Langer's solution to the problem of semblance in art is entirely original, and that one has to take it as it is, in its own terms. No modification, no tailoring of her conception is possible. There is no option of dissociating this category of virtuality from that of symbolic expression.

The category of *Schein* is derived from Kant and Schiller, especially from the latter, who taught that form and content must be treated as an organic whole. Kant had much to say about the distinctiveness of aesthetic ideas, which, in his view, do not fall under a concept. But in pondering about the symbolic as against the schematic (e.g. in *The Critique of Judgment*, par. 59) he didn't add

[1] Cf. my introduction to S. Morawski and L. Baxandall, eds., *Marx and Engels on Literature and Art* (St. Louis, Mo.: Telos Press, 1973), pp. 3-47, esp. pp. 13 and 41/2.

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much to our understanding of the problem. Cassirer went further, in elaborating the category of symbol, holding that any cognitive form is a symbolic form, and that these symbolic forms are related to the a priori structures or rules of operation of the human mind. Langer, on the other hand, speaks of the structures or symbolic forms of feeling which are expressed by and are structurally analogous to the patterns or structures which the artist has constructed in the artwork. Virtuality, then, as abstracted impersonalized form, depends on the symbolic creativity of the artist. Something is or is not an expressive symbol because it embodies to one degree or another or fails to embody this structural homology with the life of feeling. In replying to criticism (in *Problems of Art*), Langer argued that she employed the term 'expressiveness' in a quite definite way and in the only appropriate sense, as meaning living form, articulated in a specific logical structure. The reader is easily persuaded that what she has in mind is something quite different from (one could even say, *alien* to) Collingwood's concept of expression. There is really a rather long way from the Marburg school to Croce!

There remains, however, some obscurity with regard to Langer's idea of *form*. She compares it to Clive Bell's and Roger Fry's notion of "significant form," and also suggests that it is the structural shape of a thing, i.e., a materially accessible object, a whole, or *gestalt*, achieved by means of given (artistic, technical) devices. But if this is so, why doesn't the form itself, as such an objectified pattern, constitute the virtuality of any artwork? But if it does, then how can we be told, in the same book, that the principal function of art is symbolization, and that the symbolic expression takes priority over form, in constituting the artwork as semblance? These and other difficulties with Langer's conception of form—which require a much fuller discussion than I can give in this limited space—lead me to the threshold of the temptation which, as I warned earlier, would be improper and dangerous to pursue, namely, that of tailoring Langer's conception from the outside, by separating it into two parts: *form*, on the one hand; *symbolic expression*, on the other; or, the virtual form which does not require symbolization *and* the symbolized form. But how could I possibly justify this procedure? Any argument on behalf of a notion of desymbolized *form* itself would be inconsistent with Langer's view that the form is inseparable from the content, and that the paradigm of semblance or virtuality cannot be reduced to formal structuration. For Langer, whether the symbolic form is expressive, or mimetic, or both, it does not stand for something, or represent it,

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but yet it remains fully involved in the cognizance of the world beyond the artwork. The objection that immediately springs to mind is that many objects with deserved merit are acknowledged as artworks and fall under Cassirer's rubric of quality (space and time) as symbolic forms; yet they do not bear any structural homologies to the patterns of feeling, or to the felt life, as Langer views it. I am thinking of such examples as Persian carpets, Chinese porcelain, or polychrome lacquer cabinets of the Chia-Ching period, or J. Robbins' mechanical ballet, or the architectural designs of A. Loos or P. Behrens. None of these seems to me to meet the demands of symbolic form, or expressive form in Langer's sense, and yet they are artworks. I would borrow Langer's own usage to make this distinction between the *primary illusion* (which my examples exemplify) and the superimposed illusion, which opens up the avenues of the felt life.

Now one might object, in Langer's defense, that I am forcing her into an othodoxy without good reason, by insisting that the *primary illusion,* the artworks of my examples, cannot themselves be instances of symbolic form, in their own terms, even in the species of quality (space or time structure) alone and without any relation to the structures of feeling. I would insist, against this, that there are persuasive reasons to restrict the process of symbolization to the felt life, i.e., to the homology of structures of human experience (the human being's response and challenge to existential, individual, and social problems), and to such complex artistic structures as Rembrandt's painting, Bach's music, Kafka's writing. By contrast, the ideas of mere space, or time, or the contexts of space-time or of movement *do not* constitute any symbolic world. They are nothing but the embodiments of rudimentary virtuality, upon which another, enriched super-virtuality can be grounded. Thus, my conclusion runs something like this: There is evident in art an arresting difference between the nonsymbolic articulation of a self-sufficient artworld and that in which there is a symbolic correspondence of art and feeling. Semblance, virtuality, on the one hand, and symbolic expression, on the other, can be separated. Symbolic expression can enhance semblance, but the latter does not necessarily imply the felt life. For I do not find that all artworks are counterparts to the "vital process" or to "man's fate" or to some mode of sentience. But if this is so, then semblance, *Schein,* is the primary virtuality of art and its constitutive factor, but in a narrower sense than in Langer's conception of it, which, by contrast, may then be called *expressive form.*

Thus primary virtuality, semblance in this restricted sense, ap-

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pears to me to define what art is. Let me confront this narrowed conception of Langer's category of virtuality with the views of Adorno and Danto, as I suggested earlier I would do. Adorno struggled with the problem of semblance throughout his intellectual life. In his 1938 essay on fetishism in music, he counterposed the notion of virtuality to what he took to be the regressive counter-artistic production of entertainment or of jazz corrupted and transformed into commodity or exchange-value, and used to repressively manipulate an audience's emotions. In his *Aesthetische Theorie* (published posthumously), Adorno's duel with modernism is summarized: Art, we read, is always cognitive, but its truth content remains ever equivocal, a riddle not only to even the best critic, but to the artist himself. This manifold and flexible meaning of the artwork is enshrined in the interactions of *Erscheinung* and *Geist*, of semblance and spirit. The Hegelian concept of semblance is here assimilated by Adorno, and understood as revealing the untruth of the pathogenic world around us. Modern times (Adorno goes on) have engendered two polar attitudes: either of subordination to an alienated reality, or a constant counter-alienation. Art, in Adorno's view, is one of the main ways of achieving at least a partial victory in this contest. Why? Because, thanks to its virtuality, to the semblance which by its negation of what goes on in the muzzled world of reality and through which it unmasks the *Unwahrheit*, the untruth of this reality, art shows the potentialities of another, more human reality. The paradox of this *Wahrheit der'Unwahrheit* doesn't require any political or even ideological engagement, nor does it require *mimesis*. What it needs (as is shown in the work of Schönberg, Beckett, Stravinsky, Kafka, and Ionesco), is a distinctive, innovative craftsmanship capable of producing artistic structures alternative to the *entzauberte und entkunstete Welt*. Of course, Beckett's alternative vision cannot be reduced to what I called (following Langer) primary virtuality. It is much more than that, and it is precisely this "*Mehr als Form*," this something more than form, that, in Adorno's view, makes the author of *Endgame* the perfect visionary of our atrocious and delirious times. Nonetheless, it is fundamentally *Schein*, or semblance, which constitutes the realm of art and which provides us the blessed opportunity to oppose the history of our own time, which is so full of cruelty and nonsense and which deprives existence of any meaningfulness.

Adorno did not surpass the limits of the theater of the absurd, and Beckett epitomized for him the present-day crisis of art robbed of its "auratic" character. By contrast, the object of Danto's analy-

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sis is already of a historically more advanced sort. The newest *avant-garde* in its initial period—"pop art"—is his frame of reference. His solutions to the problem of art also seem to me more powerful, and clearly distinct from those of Langer and Adorno. For Danto, the artwork is not distinguished by technique or medium, nor by resemblance to an external reality (or its imitation), nor by any combination of perceptual qualities. The procedure of bracketing is, in Danto's perspective, totally opposite to the Husserlian one. There, it leads *zur Sache* itself, to the noematic essence grasped by the intellect. Here, it is an appeal to theoretical and historical knowledge—more precisely, to the artistic self-consciousness viewing the world in a certain way. Danto modifies to a considerable extent the institutional theory of art of which he was supposed to be the protagonist. Candidacy in the class of artworks has to be deserved, and the honors of election are, he states, of an ontological character.

Now what interests me here is the degree to which Danto's solution hangs on the category of semblance. Can his idea of metaphoric transfiguration of the commonplace be interpreted into Langer's concept of the primary illusion, of virtuality? Forgoing any critical discussion here of Danto's view (for lack of space), I will simply say that it seems to me that the virtuality fundamentally associated with the notion of *Schein*, or of *Erscheinung* (in contexts of perception) returns here again in Danto's account, like a boomerang, but in a different guise, that is, in what I would call a *semi-form*. The pop-artworks that Danto discusses are not to be judged simply as trite replicas of the corresponding objects they are presumed to represent. The bed, the lipstick, the diagram of Cézanne's composition, are remade, in pop art, and put at some distance from the trivial reality. This intensional bracketing produces a kind of "as if." It implies "appearance" versus what is there in the world of everyday banalities. And thus, the question of the artist's conceptual attitude and conceptually mediated aesthetic response is inevitably raised.

Danto is right in proposing that there is a rhetoric here, involving beliefs in what art could be, or should be. Art is understood as being metaphoric, as the personal expression within a given period with its immanent aesthetic self-awareness, as founded on *semi-virtuality*. This takes place, according to Danto, even if we simply arrange a real chair in real space (Kossuth) or send for exhibition dried excrement in a plastic bag (Manzoni), etc.—instances which, by the way transgress the ambiguity of Duchamp's challenge. For,

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in these radical or extreme instances, no one dare make a claim to aestheticize the presented object, and what takes place is a disclosure of the intensional bracketing, treating the presented object only as its index. Intellectual timidity retreats from drawing the correct consequences of this fact, namely that art's rival status (with regard to what we accept as natural or social realities) cannot be circumvented or canceled. However, in Danto's case, this virtuality of the artwork is not identified as a special sort of articulated structure, an expressive form; nor is it simply the mind's figment. Rather, the space of any pop-artwork must be acknowledged to be a kind of semi-semblance, not in the sense of a perceptually given self-sufficient whole, but rather as the *idea* of art's non-identity with the world around us, and with the *appreciation* of what is worthy or unworthy in a given culture. If I interpret Danto correctly, the form belongs to the rhetorical mind, not to the objects or things which are only tokens or indices of the artists', recipients', critics' possible attitudes to their historical time and their communal existence. I do not subscribe to all of Danto's conclusions, though I cannot pursue my disagreement here. Yet, it seems clear to me that his approach to the question of what art is does not break ties with the aesthetic tradition that Langer stands for.

If my confrontation of the three conceptions generated by three different outlooks does justice to them, we are justified in affirming that Langer's philosophy of art has stood the test of several decades of stormy aesthetic dispute; further, that the impact of her ideas extends far beyond the aura of the fifties, when the star of her aesthetic thought was most brightly lit. Whatever difficulties I may have with the notion of symbolic form, or with what she means by the "felt life," it seems to me that her view that semblance is the main constituent of art holds true. Let me add that there is also a striking parallelism between Langer's understanding of the significance of the philosophy of art, and of how philosophy of art interacts with art itself, and the views on this question that Adorno and Danto hold. My own ideas on art as semblance, on the most recent *avant-garde* as situated *beyond* the virtuality of the art-object (thus, beyond the semi-form of pop-art, as verging on virtuality)—all this has to await the appearance of the longer essay of which this is a part. So too does my criticism concerning the limited application of Langer's main category. (For one thing, it does not provide a key to the newest "anti-art" movements.)

I remain uncertain whether my disagreements with Suzanne Langer do not rest on my misunderstanding. But I hope that this fragment of my fuller reflections on her work will be received by

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the author of *Feeling and Form* as it was intended: namely as a tribute to her unquestionable philosophical achievements, which are alive, and inspire present-day minds to dig further and deeper in the soil she explored.

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CLASSICAL AMERICAN PHILOSOPHY: A REFLECTIVE BEQUEST TO THE TWENTY-FIRST CENTURY*

Real culture lives by sympathies and admirations, not by dislikes and disdains; under all misleading wrappings it pounces unerringly upon the human core.

William James[1]

I

The twenty-first century will not begin totally anew. Symbolically, it is a calendar milestone. Yet, it will inherit the massive problems now facing us at the end of the twentieth century. In the area of human equity, the availability and distribution of food will loom large. So, too, will the preservation of our ecosystem and the judicious use of our natural resources be at the center of our concern. The seriousness of these human problems will increase geometrically in direct proportion to the growth of world population; a growth that is foreboding and dangerous to human survival. The fact that efforts to curtail population growth are often met with opposition, either cultural or religious, does not foretell a sanguine outcome of this potential crisis. In the past, famine, infectious disease, and war acted as population-growth deterrents, historically depressing as that may be. Of these, infectious disease is now under the scrutiny of the powerful scientific knowledge of modern medicine. Further, were the affluent nations of the world to address the issue of famine with resolve and independent of political considerations, famine could also be significantly alleviated. As for war, it now has escalated into the arena of possible nuclear obliteration, which is unthinkable, although ironically we are now duty-bound to think of virtually nothing else until its specter is obviated.

*To be presented in an APA symposium on American Philosophy, December 29, 1984. Richard Pruitt will comment; see this JOURNAL, this issue, 676/7.

[1] "The College Bred," in *Memories and Studies* (New York: Longmans, Green, 1911), p. 322.

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I suggest, then, that we take the following course of action. It would be wise for us to probe our past and render as a bequest our wisest, albeit most critical tradition, that of Classical American philosophy, particularly but not exclusively, as found in the thought of William James, Josiah Royce, and John Dewey. I do not see this tradition as salvific, but I do believe that its wisdom is directly related to the problems facing global culture in the twenty-first century.

The upshot of our immediate future has a sharp clarity to it. People of the twenty-first century will live longer, will need more food and energy, and will confront a staggering increase in population as well as an increased strain on our already beleaguered ecosystem. Modern technology and modern medicine have the capacity to remedy both famine and disease, thereby still further increasing the number of people who will live and live longer. Obviously, nuclear war is intolerable as a resolution of the problem of overpopulation. Thus we are faced with more of us needing more. Consequently, more of us must share more and do more if all or, in the long run, any of us are to survive in a genuinely human way.

The above scenario, I trust, is neither startling nor even new to the listener or reader of this paper. The literature of prognostication, complete with charts of extrapolation, is vast and easily available. Nations now struggling for economic survival such as India, Mexico, Kenya, and Bangladesh will double, triple, and even quintuple their population by the year 2030, when, by conservative estimates, the population of the world will reach seven billion human beings.[2] The sophisticated and noncommercial film, "The Last Epidemic," as sponsored by Physicians for Social Responsibility, details the grim, no-win results of even a one-megaton nuclear bomb exploding over the city of San Francisco.[3]

Now, despite an occasional optimistic futurologist or the pronouncements of the politically motivated pollyanna predictions of the Hudson Institute as represented by the work of the late Herman Kahn, it does not take an intelligent person very long to ascertain that serious trouble clouds our collective future. In addition to the usual run of human vices, which will always be with us, we seem to have run afoul of a virulent case of *hubris*, or stubborn pride. The Greeks, as early as Homer, diagnosed this fault as the most serious of human failings, for it prevents healing even when com-

[2] Cf. Robert McNamara, "Time Bomb or Myth: The Population Problem," *Foreign Affairs*, LXII, 5 (Summer 1984): 1107–1132.

[3] Cf. Robert Jay Lifton and Kai Erickson, "Nuclear War's Effect on the Mind," in Lifton and Richard Falk, eds., *Indefensible Weapons* (New York: Basic Books, 1982), pp. 274/5.

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promise is available. Both Judaism and Christianity focus on the severity of the sin of pride, and western literature from Augustine to Dante, to Shakespeare, to Goethe, details its baleful consequences. The twentieth century, succumbing to the jargon of the social sciences, has a tendency to mask human failing, human vice, human sin, with a set of vague, neologistic euphemisms. Thus was born the term 'ideology'. In my lexicon, ideology is but a name for *hubris,* for stubborn pride, for Achilles sulking in his tent, for adolescent, self-destructive protest, for self-preening, and for citations from one scripture or another to justify miscreant behavior. Above all, *hubris* is a name for our refusal to face our own finitude, the imminence of your death and my death. I grant that my approach is classical, traditional, and more hard-hitting than the pusillanimous vagaries of social-science terminology. Yes, I believe in sin. Yes, I believe that human beings are potentially dangerous, to themselves, to their self-interest, and to others. Yes, I believe that human beings are seduced by nation, by turf, by religion, by the alleged sanction of cultural longevity, by the primacy of their own language, and by whatever else they can elicit as a cause to inflict damage on other human beings who have the same loyalties but to different Gods and traditions. In this regard, ideology, be it religious, social, political, ethnic, racial, linguistic, regional, or cultural, is profoundly dehumanizing, for it separates the human community into hostile and even warring camps of an irresolute nature. The paradox, however, is that the opponents in these conflicts never face up to the irresolute character of these disputes, holding rather to the need for closure, for a final solution.

Ideology would be a mere mask for conflict if it were not for the underlying drive for closure and total solutions. Despite the obvious plurality of positions taken and deeply believed by countless human gatherings, we still have a tendency to view all others as out of step if they do not emulate our values and our beliefs. Unfortunately, there are good historical reasons for this depressing state of affairs. The stark and startling residual wisdom of our collective past tells us that, for the most part, by far, all great and powerful historical movements have been on behalf of a certitude, on behalf of a definite and often exclusive goal, that is, an eschatology. And, conversely, rare has it been for the multitude to devote themselves to a cause whose message was the amelioration and celebration of the finite, the generational, the transient. This historical contrast also reveals that the pursuit of absolutes generates moral self-righteousness, fanaticism, violence, and a species of our time, global terrorism.

It would seem that attempts to effect significant progress in re-

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sponse to the problems facing us in the next century will have to proceed from a more multiple set of sources and feature a basic change in attitude with regard to both possibilities and frustrations. This attitude will have to be more pluralistic, more tolerant, and less committed to ideology, closure, and imposed solutions. In effect, we need less manipulation and a deeper understanding of the pragmatics of step-by-step healing. This will involve a different metaphysics; that is, a move from substance to process as the major metaphor of explication and a heightened sense of the finite in all our endeavors. In my judgment, the speculative bedding for this change in attitude was begun in nineteenth-century America by Williams James, continued in the *late* thought of Josiah Royce, and brought to fruition by John Dewey.[4]

Despite differences in philosophical style, terminology, and some disagreement, it is an accurate generalization to say that these thinkers were committed to pluralism, a positive form of provincialism, tolerance, meliorism, a pragmatic epistemology and ethics, the necessity of mediation, and a skepticism about any form of ultimate salvation external to the human effort. Our future, therefore, does not await some natural or divine *deus ex machina,* but rather hangs in the balance, awaiting the outcome of our deliberate, willing, and intelligent efforts to remediate, to ameliorate, to heal and to blunt and shunt those destructive forces which seem inevitably to accompany our journey. Profoundly rooted in the American psyche, these thinkers, nonetheless, had a deep and learned sensibility for cultures other than their own. The tradition of Classical American philosophy is well worth reconsidering as an ally in our entrance to the complexity of the twenty-first century.

II

In speaking of Classical American philosophy, George Santayana, himself notoriously ambivalent about America, points to the originating power of that tradition.

> When a way of thinking is deeply rooted in the soil, and embodies the instincts or even the characteristic errors of a people, it has a value quite independent of its truth; it constitutes a phase of human life and can powerfully affect the intellectual drama in which it figures.[5]

[4] The doctrines of fallibilism and tychism in the thought of Charles Sanders Peirce, the social criticism of George Santayana, and the social philosophy of George Herbert Mead are also of moment here, but treatment would require a paper of far more than the allotted space.

[5] *Character and Opinion in the United States* (New York: Doubleday Anchor, 1956), p. 20. A further gloss on the philosophical articulation of the American native soil is found in George Herbert Mead, "The Philosophies of Royce, James and Dewey in Their American Setting," *International Journal of Ethics,* XL (1929/30): 211-231.

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It is this double aspect of being rooted in one's culture, while yet providing a philosophical articulation that is transcendent of that culture, which characterizes the influential philosophical movements of the past. One is especially struck by the capacity of the ancient philosophers to turn philosophical speculation about their immediate situation into virtually timeless insights relative to the human condition. Certainly, this is the abiding strength of Plato, Epicurus, Lucretius, and Marcus Aurelius, each of whom bequeaths a moral epistemology, a fusing of knowing and doing so as to live wisely and fruitfully. This is the perspective from which I view Classical American philosophy, one in which reflection and action are continuous, of a piece, and in constant need of each other. Following John Dewey, "reflection is native and constant."[6]

The most salient contention of the American philosophical tradition herein under consideration, is the steadfast belief in the finite, transitory, and malleable character of the human journey and the accompanying habit of being chary about extra-experiential resolutions of our problems. The tradition takes the finite at dead-reckoning,[7] complete with its irresolute character. James was a lifelong opponent of absolutes and of any appeal to solutions that transcended human experience, although his doctrine of experience is the richest and most embracive ever proposed by a major philosopher. Royce and Dewey were early enamored of the Absolute Mind, coming to consciousness as they did under the eaves of late nineteenth-century Protestant Hegelianism. By the turn of the century, however, primarily owing to the influence of James's *Principles of Psychology,* they turned to a more temporalist context for their metaphysics and especially for their social philosophy. Each thinker, in his own way and at different times in their personal[8] and intellectual lives, came up against the stubborn presence of mishap. What is crucial here is that neither James, Royce, nor Dewey was convinced that a post-human, post-historical resolution was in the wings. To the contrary, they set out to plot a way by which we can forge a response to the aberrant, to the threatening, and to those forces which prevent us from taking hold of our situation in

[6] "The Need for a Recovery of Philosophy," in Jo Ann Boydston, ed., *The Middle Works, 1899–1924,* vol. 10: 1916–1917 (Carbondale: Southern Illinois UP, 1980), p. 6.

[7] In the phrase, "takes the finite at dead-reckoning," the philosophical pun is intended, for it is our unrequited death that is the ontological bane of our existence, and yet it must be reckoned.

[8] It is of note here that aside from other afflictions, James lost an infant son, Dewey lost an infant brother, two young children, and an adolescent grandson, whereas one of Royce's children was a victim of severe mental illness. Offenses to the innocent tend to drive one into the bosom of an alleged Infinite or into the potential healing of the shadows of time. They took the latter route.

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a way that is placating, healing, and ameliorative. Given the fact that we shall not be rescued from afar, the passing of time, human hopes, foibles, and dreams become the stuff of nothing less than ontology.

In the light of western metaphysics, this commitment to the affairs of time obviously occupies a lesser place. The celebration of human experience, especially in its perceptual rather than conceptual activity, strikes a blow at the long-standing assumption as to how one obtains philosophical wisdom. There are predecessors, the Greek and Roman Stoics, for example, but in the main, a belief that everyday experience, perceptually undergone, is the touchstone for metaphysics, constitutes a minority voice in the cacophonic history of western philosophy. Yet, this was precisely what the American classical philosophical tradition was proposing, namely, that the very transient character of our human lives enhanced, rather than denigrated the profound inferential character of our values, decisions, and disabilities.

In phenomenological terms, beginning with William James, the task was to diagnose the experience of the individual as we cast about in the flow of experience, bringing a mind-set but also getting and begetting as the press of the world filtered into our consciousness. Royce and Dewey took the promethean self as prescribed by James as an ideal, but they were more sophisticated about the social fabric in which this self came to consciousness. They undertook the task of proposing a community in which the individual would have the flair and personal creativity of the Jamesian self, yet would also be able to manage the complex demands, obstacles, and conflicts inherent in a social setting. This diagnosis of the human self as engaged in the flow of events resulted in a high sensitivity to novelty, to the inordinate lack of clarity on important issues, and an admission, especially by Dewey, that our situation is ineluctably problematic. Given this, breakthroughs and occasional consummations are possible, but they too signal the beginning of still another knot confronting us in our journey. The worthiness of our alleged insights, our claims to the truth, is only as significant as their ability to sustain themselves in the consequences that follow. James writes that "There can *be* no difference anywhere that doesn't *make* a difference elsewhere".[9]

The building of a human life, a community, a nation, a world,

[9] "Pragmatism", in Frederick Burkhardt, ed., *The Works of William James* (Cambridge, Mass.: Harvard, 1975), p. 30. Parenthetical page references to James will be to this paper, this edition.

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or perhaps in the future a cosmopolis, requires that we acknowledge from the outset the permanent relationship between growth and setback. The world and its creatures are not a "block-universe," inert substances awaiting definition and management. Rather the world comes to us as a delicately woven fabric, related in a myriad of ways, and, although susceptible to human change, it carries with it its own set of affairs. Human intrusion into the affairs of nature or into other, alien human affairs, generates a response, consequences. The pragmatic method tells us that the worth of our intrusion is tied directly to the worth of the consequences and not to the clarity of the coherent conceptual scheme that originated the involvement. Nor is the worth of our intrusion tied to our self-justifying pronouncements from scriptural sources, whether they be religious or political. For they too must weigh the consequences. Further, as we have now learned at great possible peril to ourselves, consequences do not necessarily show their hand immediately. Often, to the contrary, they remain quiescent, buried in the flush of apparent success, and then, in time, their time, they erupt with a vengeance. Compare, for example, the macho sense of power which came upon the heels of the bombs at Hiroshima and Nagasaki, with the sense of terror that now afflicts citizens of the globe as they ponder the consequences of the decision to open that Pandora's box. Countless other examples abound in the areas of pharmacology, pesticides, urban renewal projects, pollution, military aid, and technological devices of every stripe.

At bottom, pragmatic epistemology is an attitude; one that does not make truth announcements, let alone pronouncements or manifestoes, but rather is a probing, an experimental probing. The pragmatic approach eschews the intuitive and the deductive as licit sources of final knowledge. They are, however, regarded as legitimate points of departure, for no claim is ruled out a priori. Yet, even the intuitive and the deductive must stand the test of consequences. Pragmatism has an inductive temper, yet it is far more aware of possible novelty and it is willing to treat ideas as explorers, ferreting out new ground on which to stand, even at the risk of being severely wrong. It was Dewey who stressed that error and failure, if properly respected, are often profound sources of insight. Pragmatism features a paradoxical combination of epistemic modesty and boldness. It is modest in that no truth claim is seen as final until the last person has his or her say and all the consequences of the truth claim have shown their hand. Pragmatic truth, then, always has a tentativeness about it, in contrast to the more

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traditional forms of truth as correspondence or coherence.[10] It is bold in that pragmatism allows truth claims to be floated without sufficient evidence, so as to intensify the search for evidence that would not surface unless the claim were proposed. Empirical evidence to the contrary will, of course, sink the claim.

For purpose of the present context, it is both the tentativeness and the experimental character of pragmatism with regard to truth claims, value judgments, and assorted efforts to tell other people what is what and what to do, which yield its ring of authenticity as an approach to patterns of future amelioration. The pragmatic approach is precisely the opposite of that characterized by ideology and claims of certitude on thorny and even long-standing issues of apparently intractable conflict. When the search for truth is experimental, probing, and open to surprises, novelty, and setback, the tendency is to take a wide view of the evidence and an even wider view of the possible participants who can be of assistance in this search.

It seems to me perfectly legitimate and instructive to draw a relationship between the pluralism of a pragmatic epistemology and the cultural pluralism of James, Royce, and Dewey. Although as Americans we often violate the commitment to pluralism, it is our most profound tradition and that which has the most direct and signal significance for the next century. World culture is not a seamless garment, knit together from a single bolt of cloth. It is plaid in fabric, with often frayed ends, missed stitches, and clashing colors. World culture is many-hued and in an often riotous arrangement. Just as our cultural maps undergo changes, some subtle, some vast, so too do the lives of the people who find themselves, invaded, captured, sold, relocated, or forced to change their external beliefs. Yet, despite these upheavals, most of them against the will of those who are subject to the intrusion, the multiplicity of languages, customs, religions, attitudes, and endless lifestyles persist. Even the bullying of the Red Guards of the Chinese cultural revolution was unable to change the concentric, thousands-year-old history of the open-market system of the Chinese. The Jewish people, the Polish people, and many others defy superpower ideology to maintain the quality and integrity of their precious traditions, both personal and communal.

Neither American economic imperialism nor Soviet expansionist communism has a clue to the majesty and the obduracy of the mul-

[10] For a clarifying and perceptive discussion of the originality of pragmatic truth as developed by William James, cf. H. S. Thayer, "Introduction," to James, "The Meaning of Truth," in *The Works of William James, op. cit.*, pp. xxv–xliii.

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tiple styles of life that constitute global culture. Because of the seduction of self-approving ideology, the superpowers, however well intentioned, show up and opine, in a paraphrase of Pygmalion, Why can't they be like us? Whatever the source, and it is usually at the behest of a gun barrel, cultural imperialism is an abomination. The task, of course, is for each culture and subset thereof to be open to the experience, originality, and value of other cultures. This is possible only if individuals are taught from the outset to be open to experience and to avoid judgments based on conceptual a priori assumptions, inherited truths legitimated only by longevity or by authority. These obstacles to genuine inquiry correspond exactly to Charles Sanders Peirce's critique in his paper on "The Fixation of Belief," wherein he chastises the methods of tenacity, authority, and of the "a priori".[11] Peirce proceeds to develop a doctrine of the community of inquiry, which stresses constant experiment and multiple participation. He early anticipated what we now take to be a truism, that the major problems that loom before us rarely will yield to the wisdom of a single observer or even to the activity of a single method or tradition. Peirce's ideal was that some day we would replace competition with cooperation in the arena of scientific inquiry. This is a goal to which we add the arena of social and political inquiry as well.

In our attempt to forge a genuinely pluralistic attitude toward inquiry and to encourage recognition of a plurality of styles and beliefs, it should be made clear that this effort is not a waiting-game until all participants agree to agree. Pluralism is not a temporary aberration in the great cosmic scheme, if there be one, nor is it a fall from grace. According to the classical American philosophers, plurality in virtually every arena of activity is a permanent characteristic of the human scene. One way to stress this is to develop a sensitivity to a positive provincialism. The negative side of provincialism is obvious. One example will suffice. Most Americans of this century have been formally educated as though most of the world did not exist. For many Americans, the knowledge of other people, other lands, and other languages is notoriously lacking. It is precisely this form of ignorance which leads to xenophobia and which conditions us to succumb to ideology.

On the other hand, human beings come from somewhere, speak a native language, and receive a host of cultural inheritances. Most of us settle somewhere—a nation, a region, even a city or a town or

[11] *Collected Papers of Charles Sanders Peirce*, vol. V, par. 377-387 (Cambridge, Mass.: Harvard, 1934), pp. 233-247.

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a village—and that place constitutes for us the lineaments of our transaction with time and space. We may travel, but we return home. "A citizen of the world" has a nice ring to it, but it belies the way we think, the provincial source of our beliefs, our values, and our attitudes. In a prophetic essay, written in 1902, Royce distinguishes between a negative and a "wholesome" provincialism, one "to which the world in the near future will need more and more to appeal."[12]

Royce stresses the need for local communities to develop a sense of pride in their origins and accomplishments. He also encourages communities to build and create a distinctive character. But then, and this is the heart of a genuine pluralism, he urges the members of the community to seek out other communities, especially abroad, and to share the values and the ideals in a common spirit of inquiry. Obviously, this does not always go well, for values and ideals are often in conflict. In order to build a community and a brace of communities and finally a world community, mediation is needed. Royce refers to this mediator as an "interpreter." Where two persons or communities are face to face, it is often necessary for a third to interpret them to each other. In this way, one preserves the integrity of one's own lineage and position and is still able to reach out and share that of the other. Contrary to Royce's earlier absolute idealism, the path trod here leads not to an absolute nor to a final community as the realization of a fixed eschatology. Royce's goal is more modest and more empirical. He seeks a way to aid us in building a "beloved" community, with the attaining virtue of hope, rather than the more ideological virtue of faith. Remarkably, he writes that "*by the 'real world' we mean simply the 'true interpretation' of this our problematic situation.*"[13] Consequently, unless both the interpreter and the community are real, there can be no real world. Further, he denies that

> . . . the real world is anything merely static, or is a mere idea within the mind of a finite self, or is an Absolute that is divorced from its appearances, or is any merely conceptual reality, or is "out of time," or is a "block universe," or is an object of a merely mystical intuition (*ibid.*, p. 339).

If we follow Royce's line of reasoning, the conclusion is inescapable. In building a human community, we are constitutive of nothing less than the world. If our community is in tatters, so too is the

[12] "Provincialism," in John J. McDermott, ed., *The Basic Writings of Josiah Royce*, vol. II (Chicago: University Press, 1969), p. 1069.
[13] *The Problem of Christianity* (Chicago: University Press, 1968), p. 337.

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world. If our community is self-deceived and illusory, so too is the world. If our community is on-going, integral, and ameliorative, so too is the world. Royce has transformed the traditional idealist position, which reads that "the world is as it is thought," to "the world is as it is built to *be*." It is not the goal so much as the quality of the interpreted journey which Royce bequeaths to us as the meaning of individual life, inseparable from communal life.

We have no guarantee that any of this will go well, although it is well worth attempting. Put differently, neither optimism nor pessimism seems to be a proper response, for each approach neglects the data generated by the other. Yet, it is characteristic that we are called upon to favor one response or the other, as in the rhetorical question, Are you an optimist or a pessimist? James seeks to avoid such a dichotomy when he writes:

> Ought not the existence of the various types of thinking which we have reviewed, each so splendid for certain purposes, yet all conflicting still, and neither one of them able to support a claim of absolute veracity, to awaken a presumption favorable to the pragmatistic view that all our theories are *instrumental,* are mental modes of *adaptation* to reality, rather than revelations or gnostic answers to some divinely instituted world-enigma? (94)

Following James, neither optimism nor pessimism provides the adaptive ability required in our approach to the human situation, for they tend to be cut off from the reality of setback and possibility, respectively. Rather, the strategy for effecting concrete and beneficent change should follow the moral attitude of meliorism, that is, simply, to strive to make things better. There is no illusion here about the possibility of perfection, but there is a belief in the growth of sensitivity, of goods, and of enjoyment, especially the aesthetic, as having to do with living in an environment that is celebratory, adorned, and able to combine tradition and novelty. For James, pluralism and meliorism are entwined; the first celebrates diversity whereas the second aids in the development of originality and adjudication of conflict between a variety of styles. James sees the relationship between meliorism and pluralism as follows:

> The melioristic universe is conceived after a *social* analogy, as a pluralism of independent powers. It will succeed just in proportion as more of these work for its success. If none work, it will fail. If each does his best, it will not fail. Its destiny thus hangs on an *if,* or on a lot of *ifs*—which amounts to saying (in the technical language of logic) that, the world being as yet unfinished, its total character can be expressed only by *hypothetical* and not by *categorical* propositions. . . .

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> As individual members of a pluralistic universe, we must recognize that, even though we do *our* best, the other factors also will have a voice in the result. If they refuse to conspire, our good-will and labor may be thrown away.[14]

James is correct about the risk involved in any attempt to effect significant change. The effort must be carried on with a delicacy worthy of the pluralism and the positive provincialism it espouses. The risk is compounded if the agent of change is naive about the twin, always allied requirements for successful amelioration. The first requirement we have stressed in this essay, namely, an abandonment of fixed positions, ideological cant, and a priori judgments. This can be brought about only by what Jewish tradition calls a *Teshuvah*, a change of heart. This is a moral epistemology, which is rooted in Plato and found recurring in the Stoics, Augustine, Spinoza, Bergson, Buber, and especially in Classical American philosophy. I foresee no significant social or political amelioration without this "turning."

The second requirement for successful amelioration has not been discussed here and remains for another setting. I refer to the need for sophistication as to how institutions deflect, co-opt, and dilute change, while all the while praising its presence. This warning against massive self-deception is usually attributed to Marx, and correctly, but John Dewey was well aware of its presence. In 1935, Dewey spelled out the need for sophistication as to how good works spin their wheels when subjected to the wiles of institutional survival:

> The conditions that generate insecurity for the many no longer spring from nature. They are found in institutions and arrangements that are within deliberate human control. Surely this change marks one of the greatest revolutions that has taken place in all human history. Because of it, insecurity is not now the motive to work and sacrifice but to despair. . . .
>
> The educational task cannot be accomplished merely by working upon men's minds, without action that effects actual change in institutions. The idea that dispositions and attitudes can be altered by merely "moral" means conceived of as something that goes on wholly inside of persons is itself one of the old patterns that has to be changed.[15]

Global amelioration, then, is possible under the following conditions: (1) awareness of how institutional change takes place, *mu-*

[14] "Faith and the Right to Believe," in McDermott, ed., *The Writings of William James* (Chicago: University Press, 1977), p. 739.
[15] "Renascent Liberalism," in McDermott, ed., *The Philosophy of John Dewey*, vol. II (Chicago: University Press, 1981), pp. 646/7.

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tatis mutandis, in different societies, that is, an avoidance of the innocence or the naivete that thinks that good will is a sufficient agent of change; (2) a profound awareness of the cultural heritage, values, and taboos of other societies as well as an awareness of what they can in conscience yield and what in conscience they must protect; (3) an abandonment of ideology and a concentration on shared values; (4) the willingness of the technologically affluent and politically powerful nations of the world to accept comparable suggestions for amelioration of their own weaknesses and offenses. Perhaps then we, the nations of the world, could join in an effort to develop Royce's "beloved community", while keeping an eye directly on famine, population, ecology, and nuclear war. Without an attenuation and developing amelioration of those four problems, the beloved community is a chimera. I believe that we can make a "turning" and begin again. William James says it best:

> If we survey the field of history and ask what feature all great periods of revival, of expansion of the human mind, display in common, we shall find, I think, simply this: that each and all of them have said to the human being, "The inmost nature of the reality is congenial to powers which you possess."[16]

In this text of James, the fundamental dialectic of our situation is laid bare in one trenchant sentence. Reality has its givenness, its obduracy, its nature. We, human life, have our powers. Neither triumphs for the good of either. It is congeniality we seek, that is, a community of inquiry, characterized by cooperation, insight into the needs of the other and liberation from arrogance. All philosophy, one way or another, is helpful to us in our plight. Yet, Classical American philosophy seems to be closer to the mark. We owe it to ourselves to rejuvenate this tradition as an intelligent source for our future global activity. It is very likely that a copy of William James's *Will to Believe* may be more salutary for the future of my grandchild and his peers than still another missile complete with a nuclear warhead. Philosophy bakes no bread, but it knows white from rye. Or should we wait for attrition and demolition before the plaintive cry reaches to heaven for vengeance or redemption, neither of which, if world history be our guide, will be forthcoming?

As for me, I am on the side of trying!

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[16] "The Sentiment of Rationality" in "The Will to Believe," *The Works of William James, op. cit.,* p. 73.

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THE BIASES OF PLURALISM*

FOR whatever reasons, the liveliest philosophical writings of the past few years have variously advocated radical revisions of philosophy. Though not all the authors of these texts regard themselves as pragmatists, their arguments are typically organized around two points which have always been essential to pragmatism: the rejection of the search for methods to achieve cognitive certainty by means of epistemological foundationalism ("cartesianism"); and the requirement that philosophical discourse have something interesting, important, and helpful to say about the contemporary human situation. These recent texts provide demanding, engaging, and provocative methodological discussions, but are, in various ways, banal or otherwise disappointing when they come to characterize the general human significance of their technical devices. Professor McDermott, on the other hand, somewhat reverses the apparent priorities. He accepts without much analysis pragmatism's hundred-year-old rejection of cartesianism, preferring to satisfy first the demands for interesting human applications. He regards pragmatism's pluralism, meliorism, and acknowledgment of individual and cultural finitude as those philosophically articulated attitudes which are urgently relevant for the coming world situation.

We must remember that the pragmatism with which McDermott is concerned was originally constituted in various ways as a kind of cultural salvage operation. Classic American philosophy was basically a philosophy addressed to groups of once powerful culturally besieged middle-class Protestants whose ties to a supportive tradition had become problematic in the well-known identity cum spiritual-social-political-economic crises of the latter part of the nineteenth century. The quests for community by John Dewey and Josiah Royce should be regarded as philosophically updated attempts to maintain traditional Protestant beliefs. William James's *Pragmatism* lectures are the clearest example of this project. It is there that pragmatic consequentialism is spelled out as a principle of demystifying interpretation. Troubled Protestant beliefs are translated into untroubled Protestant desires. Belief in God *means* desire for human salvation. Meliorism replaces cosmic optimism in a way precisely compatible with Northeast American Unitarianism.

*Abstract of a paper to be presented in an APA symposium on American Philosophy, December 29, 1984, commenting on John J. McDermott, "Classical American Philosophy," this JOURNAL, this issue, 663–675.

0022-362X/84/8111/0676$00.50
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Pragmatism, then, became significant, generally interesting, and productive in the context of apparent cultural or ideological breakdown. It serves as a principle of interpretation that reveals the deeper practical currents underlying human belief. As James said, it "loosens" our theories by revealing their genesis in practical desires that may be variously manifested and satisfied. The "hardened" theories become temporalized, humanized, desacralized. If we regard beliefs as functions of practical desires, and practical desires as based upon the sense of possibilities and accommodating and interfering powers in the world, we should regard a culture as organized around beliefs regarding the nature and sources of power, the possibilities of human life, the catalogue of human desires and the devices for satisfying those desires. Insofar as modernization and cross-cultural contact bring about radical tensions in cultures so characterized, pragmatism of some sort may be of vital importance for staving off cultural disintegration and ideological defensiveness. Insofar as the next century will witness an accelerated international modernization and cross-cultural tensions, pragmatism, as McDermott asserts, appears to be the appropriate distinctive American bequest to the twenty-first century. It can achieve cultural change without radical cultural anxiety.

I say that the gift of pragmatism "appears" to be appropriate because the pluralism, meliorism, and tolerance that pragmatism espouses are not clearly universally adoptable options. I suggest that, rather than being an American doctrine that transcends American culture, pragmatism is a continuation of a nontranscending American Protestantism. James acknowledged it as such. Pragmatism becomes acceptable as a continuation of Protestant doctrines organized around an individualist epistemology. It is, in short, an ideology. Pluralism, meliorism, and tolerance are important elements of this ideology. We should realize that a plurality of beliefs, in itself, is no sufficient (or necessary) ground for official pluralism. Nor is the relativity of belief necessarily an adequate ground for official relativism. And tolerance, after all, is a political, not a cognitive or ontological notion. The gift of pragmatism to non-Protestant nations, then, becomes an invitation to conversion, or disintegration—an invitation that needn't recognize itself as such. But, after all, what entrenched ideology does adequately recognize itself as such?

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THE DEBATE ABOUT MENTAL IMAGERY*

THERE has been much debate recently among cognitive psychologists about the nature of mental images. Some psychologists, notably Stephen Kosslyn,[1] maintain that mental images represent in the manner of pictures, and others, notably Zenon Pylyshyn,[2] maintain that mental images represent in the manner of language. The former view has come to be called *pictorialism*; the latter, *descriptionalism*. Philosophers have entered the debate with arguments on both sides.[3] My purpose, in this paper, is to challenge one of the foundations of this debate. In my view, the experimental data on mental imagery can be explained perfectly well without making the basic assumption of both pictorialists and descriptionalists, namely that *there are*, in the brain, mental images that persons have when they undergo image experiences. I also believe that this assumption is suspect on other grounds, and I shall advance a theory of imagery that avoids mental images altogether.

The structure of the paper is as follows. In section I, I sketch an adverbial analysis of mental imagery. In section II, I discuss various pieces of empirical data on imagery, and I argue that they can be explained by the adverbialist. In section III, I summarize various theoretical reasons for and against the view that there are mental images, either in the brain or in some nonphysical realm, and I explain why I prefer the adverbial approach.

I

There can be little doubt that the ways in which we speak suggest that images are objects which we apprehend or have in experience. We say "The image I am experiencing is red and fuzzy at the edges," "I have an image of a striped tiger," and so on. According to the adverbial theory, the grammar of these statements is system-

*To be presented in an APA symposium on The Debate about Mental Imagery, December 29, 1984. Barbara Von Eckardt will comment; see this JOURNAL, this issue, 691-693.

Parts of this paper were written during a visit to Oxford University, I am grateful to the fellows of Hertford College for their hospitality.

[1] For a summary of Kosslyn's work, see his *Image and Mind* (Cambridge, Mass: Harvard, 1980).

[2] See, for example, Pylyshyn, "Imagery and Artificial Intelligence," Ned Block, ed., in *Readings in the Philosophy of Psychology*, vol. 2 (Cambridge, Mass: Harvard, 1981); also "The Imagery Debate," in Block, ed., *Imagery* (Cambridge, Mass: MIT Press, 1981).

[3] For defenses of pictorialism, see, for example, Block, "Mental Pictures and Cognitive Science," *Philosophical Review*, XCII, 4 (October 1983): 499-542, and "The Photographic Fallacy in the Debate about Mental Imagery," *Noûs*, XVII, 4 (November 1983): 651-661. Arguments for descriptionalism are given by Daniel Dennett.

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atically misleading. The general point here is familiar enough: grammatical form need not coincide with logical form. Still, if our talk of images is not to be taken at face value, then how is it to be understood?

The basic claim of the adverbial theory is that statements like

(1) James has an image of a bald man.

are logically on a par with statements like

(2) James has a marked limp.

rather than statements like

(3) James has a loving mother.

Thus, just as (2) can be rewritten as

(2a) James limps markedly.

so (1) may be recast as

(1a) James images a-bald-man-ly.

Once (1) is reformulated in this way, we see that having a mental image is a matter of imaging in a certain manner rather than owning an image object. Relatedly we see that mental images, like limps, do not really exist.

The adverbial analysis outlined here is in immediate need of further clarification, however. What is the meaning of the adverb 'a-bald-man-ly'? What exactly is the logical form of (1a) and other adverbially modified imaging statements? What are the metaphysical commitments of the adverbial analysis of mental imagery? I have developed the adverbial theory in detail elsewhere, and it would be redundant to repeat my discussion.[4] Nonetheless it will be useful to have before us a summary of the basic tenets of my version of the adverbial theory, as it applies to (1a) and other such statements.

(A) Statements like (1a) are analyzed as quantifier-free subject-predicate statements, the predicates of which are semantically compound. The basic idea here is that adverbs are treated as predicate operators. Syntactically, these operators precede predicates, thereby forming more complicated predicates. Semantically, they can be viewed as functions that take the properties expressed by the predi-

[4] See my "The Adverbial Approach to Visual Experience," *Philosophical Review*, XCIII, 2 (April 1984): 195–226. For a defense of the adverbial theory in connection with bodily sensations such as pain, see my "Pain and the Adverbial Theory," *American Philosophical Quarterly*, forthcoming in October, 1984.

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cates they modify onto new properties. For example, the open sentence

(4) x images redly.

is regimented as (4a) $[R]Ix$.

(4a) is made up of an operator '$[R]$' ('redly') preceding the formula 'Ix' ('x images'). The function or operation that '$[R]$' stands for maps the property expressed by 'Ix' (the property of imaging) onto the property expressed by (4a) (the property of imaging redly).

(B) Metaphysically, the above adverbial theory of imagery is committed at the level of concreta to no more than imaging persons. However, since the semantics given for (4a) introduces both properties and functions, the proposed adverbial theory is also committed to the existence of these abstract entities.

(C) Adverbs like 'a-bald-man-ly' are taken to be semantically compound, having as components adverbs like 'baldly' and 'manly'. Their connotations classify the functions they designate as functions operating on experiential properties typically instantiated in persons perceiving objects of the appropriate type—in the case of 'a-bald-man-ly' a bald man. In a nutshell, then, having a mental image of an F,G object is experiencing in much the way in which normal perceivers would experience were they to perceive an F,G object in standard circumstances.

The above sketch is very brief. Still, it is all that is needed for the purposes of this paper. For it should now be clear just how mental images are avoided by the adverbial approach to imagery. I turn next to an evaluation of the empirical foundations of pictorialism and descriptionalism.

II

I cannot possibly address all the different pieces of empirical data which have been held to be relevant to the debate between pictorialists and descriptionalists. My intention rather is to pick what I hope is a representative sample and to argue that the adverbialist can explain the data making up this sample at least as well as either of his opponents.

The pieces of data I want to discuss fall into three types: those linking mental imagery with perception, those motivating the pictorialist approach, and those motivating descriptionalism.

That the representations of mental imagery are like the representations of perception has the support of overwhelming empirical evidence.[5] For example, it has been found that, when gratings are

[5] From my adverbialist point of view, this *general* claim needs no empirical support, since it is analytic. This is not to say, however, that the psychological experi-

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held before subjects' eyes and moved away slowly, the subjects report that the stripes (on the gratings) begin to blur at a greater distance if they are vertical than if they are oblique. Exactly the same phenomenon occurs with mental imagery. That is to say, when subjects *imagine* gratings moving away from them they report that the stripes blur sooner if they are oblique than if they are vertical.[6]

This experiment, and others linking imagery and perception,[7] may *seem* to count against descriptionalism and for either pictorialism or adverbialism. But really they do no such thing. For what the experiments bring out are the many varied ways in which image experiences are like perceptual experiences. Whether these experiences are all pictorial, all descriptional, or all adverbial remains an open question.[8]

The experiments motivating pictorialism are many. I shall discuss only two, since I believe that what I say may be extended without difficulty to other experiments. The experiments I want to examine involve block-figure rotation and map scanning.[9] I begin with the former.

Consider the two pairs of block figures shown in Figure 1 below. Are the figures in (A) congruent? What of those in (B)? In an experiment designed by Shephard and Metzler,[10] subjects were shown sixteen hundred pairs of block figures and asked whether they were congruent or not.[11] It was found that, as the angular separation of the figures in each pair increased, so, in linear proportion, did the length of time each subject typically needed to respond. When asked how they reached their answers, subjects reported imagining one figure rotated so as to superimpose the other. Pictorialists claim that this experiment suggests that people can rotate mental images, and at fixed speeds.

---

ments linking imagery and perception are pointless. It is one thing to maintain that, as a matter of a priori necessity, imaging an *F* object is much like the experience that takes place in perceiving an *F* object. It is quite another to specify all the various similarities of feature which connect the two.

[6] This experiment was performed by Pennington and Kosslyn. See Kosslyn, "The Medium and the Message in Mental Imagery," in *Imagery, op. cit.*

[7] See, for example, R. A. Finke, "Levels of Equivalence in Imagery and Perception," *Psychological Review*, CLXXXVII (1980). Also R. N. Shephard, "The Mental Image," *American Psychologist*, CXXXIII (1978).

[8] In this paper I classify an *experience* as pictorial (or descriptional) if and only if there is, associated with it and owned by the person undergoing it, a mental object that is itself pictorial (or descriptional).

[9] These are the experiments Block cites as motivating pictorialism.

[10] R. N. Shephard and J. Metzler, "Mental Rotation of Three-dimensional Objects," *Science*, CLXXI (1971).

[11] Some pairs were rotations in the plane of the page, some were rotations perpendicular to the page, and some could not be made to match by any rotation.

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The map-scanning experiment[12] required subjects to study the map shown in Figure 2.

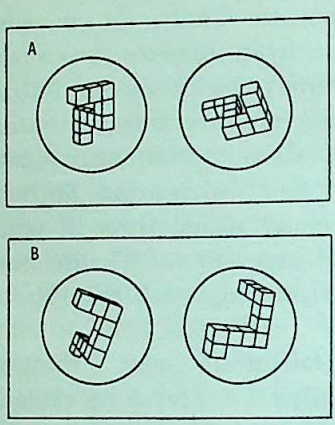

Figure 1

Figure 2

When the subjects had become familiar enough with the map to be able to draw it, they were asked to form a mental image of it and then to focus in on one particular object in the image, and this request was repeated for different objects. It was found that the farther away an object was from the place on the image presently being focused on, the longer it took to focus on that new object. So, for example, shifting attention from the tree part of the image to the hut part took longer than shifting attention from the tree part to the lake part.

Pictorialists have taken this experiment to support both the idea that mental images can be scanned and the idea that mental images can have parts of which their subjects are unconscious. This latter idea is suggested by the fact that when the subjects focused on one part of their images they were not aware of imaging other parts. Yet independent experiments on the time it takes to produce images show that their scanning times were much too fast for them to be generating a new image each time they focused in on a new object.

The general conclusion pictorialists draw from these experiments is that mental images are quasi-pictures-in-the-head which persons can construct, manipulate, and view. Is this conclusion licensed? I think not.

Consider again the block-figure-rotation experiment. If we per-

[12] For details of this experiment, see S. Kosslyn, T. Ball, and B. Reiser, "Visual Image Scanning," *Journal of Experimental Psychology: Human Perception and Performance*, IV (1978).

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mit ourselves to indulge in the fantasy that mental images are spatially extended geometrical figures which bear definite angular separations from one another and which are capable of real rotation, then we can immediately explain the results of the experiment. But, of course, the pictorialist, in claiming that the subjects are rotating mental images, is *not* asking us to indulge in this fantasy. In the pictorialist's view, mental images are neural entities having features that *represent* spatial extension and angular separation. Furthermore, image rotation is a neural process that transforms images bit by bit so that they *represent* objects at gradually changing angles. The explanation the pictorialist is, in fact, proposing, then, goes something like this: when a person imagines one figure F_1 rotated so as to superimpose another F_2, he has an image of F_1 and an image of F_2 which share a feature K representing the angular separation of F_1 and F_2. As the person rotates the former image at fixed speed so as to superimpose the latter, K systematically and smoothly changes so as to represent decreasing angular separations of F_1 and F_2. Thus the time T it takes to imagine one figure rotated so as to superimpose another increases linearly with increasing angular separation of the figures because, with greater angular separations, K has to go through more changes as the images are rotated so as to superimpose, and T, given a fixed rotational velocity, increases linearly with increasing changes in K.

It seems to me that this explanation is not in the least compelling. For one thing the adverbialist can offer a parallel explanation if he so desires. Where the pictorialist speaks of a feature K of images which represents angular separation, the adverbialist can speak of a mode M of imaging which does likewise. Given this change together with the introduction of talk of imaging rotationally, the explanation proceeds as before.[13]

A second objection is that there is an alternative, less technical, and, I think, better explanation of the results of the experiment. This explanation makes use of the same basic hypothesis as the pictorialist's account, but it remains neutral among pictorialism, adverbialism, and descriptionalism.

[13] On my adverbial approach "x rotates an image at fixed speed" becomes "x images rotationally at fixed speed." This latter construction contains iterated operators. Formally it may be written as '$[F][R]Ix$', where the operator '$[F]$' ("at fixed speed") modifies the whole compound predicate '$[R]Ix$' ("x images rotationally"). For more on iterated operators and detachment rules governing operational constructions, see my "Pain and the Adverbial Theory," *op. cit.* I should add that, strictly speaking, M is not a mode of imaging but rather a two-place operation which operates on modes of imaging representing block figures, e.g., F_1-ly and F_2-ly, thereby forming more complex modes of imaging, e.g., (F_1-ly—M—F_2-ly)-ly.

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According to the adverbial view, the hypothesis:

(5) The typical participant rotates mental images of block figures at a single fixed speed.

as it is interpreted by the pictorialist, must be mistaken, since there are no images to rotate (in any sense of the term 'rotate'). Nevertheless the adverbialist can still put forward (5) just as he can put forward:

(6) All the participants have mental images of block figures.

The point here is that (5), like (6), can be held by the adverbialist to have a misleading grammatical form. From this perspective, (5) may be recast as

(5a) The typical participant images rotationally at fixed speed.[14]

A more neutral interpretation of (5) which is consistent with (5a) but which does not assume that the adverbial analysis is correct is

(5b) The typical participant undergoes experiences much like the experiences he would undergo were he to see block figures rotated at a single fixed speed.[15]

In keeping with the intuitively plausible idea that terms applying to our inner experiences derive their sense from the terms we apply to manifest objects, (5b) analyzes the talk of rotation in (5) in terms of the rotation of public figures. By so doing, it provides us with a clear understanding of what (5) asserts. Furthermore, (5b) serves to remind any trigger-happy pictorialist that acceptance of (5) does not automatically commit us to the pictorialist view.

Now we know that the time it takes to undergo the experience that takes place in *seeing* a block figure rotated at constant speed so as to superimpose another such figure increases with increasing angular separation of the two figures. This claim, is, of course, a consequence of

(7) $$t = \frac{a}{v}$$

where 't', 'a', and 'v' represent real time of rotation, real angular separation, and mean rotational velocity, respectively. Given (5) as our hypothesis and (5b) as its analysis, we should, therefore, expect

[14] This analysis may be formalized in the manner suggested in the last footnote.

[15] (5b) requires sharpening. For example, the antecedent of the subjunctive conditional needs the following qualification: "and were he a normal perceiver located in standard perceptual conditions." I ignore this and other qualifications since they do not bear on my argument.

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that the time it takes for the typical participant in the experiment to decide whether the figures in each pair are congruent will increase linearly with increasing angular separation. And this is indeed the result we get.

This explanation strikes me as plausible. Furthermore, it can be accepted by all parties in the dispute. For (5), once analyzed as (5b), leaves open the question whether the experiences the typical participant undergoes are ultimately adverbial, pictorial, or descriptional. Hence the claim that the block-figure-rotation experiment supports pictorialism is, I believe, completely unfounded.

In the case of the map-scanning experiment, similar points apply. Thus, we can certainly concede to the pictorialist that if scanning an image were the same as scanning a real spatially extended picture then the experiment would be expected to yield the results it does. But, as the clear-headed pictorialist grants, mental images are not really spatially examined. Thus, the explanation the pictorialist offers of what is going on in the experiment needs much more unpacking than his comments about image scanning indicate.

Here is one way of coming to grips with the pictorialist's explanation. When a person has a mental image of the earlier island map and its component figures, the time it takes to shift from focusing on the tree-figure representation in the image to focusing on the hut-figure representation is longer than the time it takes to shift from focusing on the tree-figure representation to focusing on the lake-figure representation, because of the following six facts: (1) there is an image *I* of the map which the person possesses; (2) there is a process of scanning *I* which occurs whenever the person focuses on one component of *I* and then shifts his focus to another; (3) there is a feature *D* of *I* which represents the distance from the figure represented at the beginning of scanning to the location on the map represented by the part of *I* reached at any given time *t* during scanning; (4) *D* systematically and smoothly changes during scanning as the distance it represents increases; (5) the functional relationship in (4) together with the fact that the tree figure on the map is nearer to the lake figure than to the hut figure ensure that *D* passes through more changes during image scanning from tree to hut representations than during image scanning from tree to lake representations; (6) the time of scanning increases linearly with increasing changes in *D* (assuming a fixed scanning speed).

This explanation, like its predecessor in the block-figure-rotation experiment, does not succeed in motivating pictorialism over adverbialism. For again the adverbialist can construct a parallel

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adverbial explanation of the experiment.[16] Another criticism of the pictorialist's claim to have motivated his position is that, as before, it seems to me that there is a satisfactory explanation of the data which assumes none of the three positions under discussion. I shall now sketch this explanation.

It seems very natural to describe what goes on in the map-scanning experiment in the following general way:

(8) Each participant focuses in on given parts of his image of the map and then shifts his focus to other parts (in response to instructions).

I believe that (8) may be analyzed (in first approximation) as

(8a) Each participant undergoes experiences much like the experiences he would undergo were he to see given figures on the map and then shift his gaze to other figures (in response to instructions).[17]

This analysis, like the earlier analysis (5b) of (5), strikes me as intuitively plausible. Furthermore, (8a) serves to emphasize the point that (8) itself does not automatically entail the pictorialist view.

Now, following the lead of the pictorialist, we can hypothesize:

(9) When the typical participant shifts his focus from given parts of his image of the map to others he scans across his image at a single fixed speed.

But, unlike the pictorialist, we can maintain that (9) asserts no more than

(9a) When the typical participant shifts his focus from given parts of his image to others, he undergoes experiences much like the experiences he would undergo in seeing were he to move his line of sight from figure to figure on the map at a single fixed speed.

[16] On the adverbial approach, scanning across a mental image (of the map) from part A_1 to part A_n, that is, focusing on part A_1 in the image and then shifting one's focus "along a line" to part A_n, may be analyzed as instantiating through time, one after another, the members of the following sequence of properties: imaging A_1-ly focally, imaging A_2-ly focally, imaging A_3-ly focally, . . . imaging A_n-ly focally. Within this sequence, any two adjacent modes between A_1-ly and A_n-ly represent just noticeably different locations on the map situated on a line connecting the locations (or figures) represented at the beginning and end of scanning. Since the number of different imaging properties making up the above temporally extended imaging sequence increases linearly as the distance represented during scanning increases, it follows that the time of scanning across a mental image increases linearly with increasing distance on the map.

[17] (8a) needs to be qualified in the same sort of manner as (5b) earlier (see fn 15). The same is true for (9a) and (11a) below.

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With the formulation of (9a) we are in a position to explain the results of the map-scanning experiment.

We know that the time it takes to move from seeing one figure on the map to seeing another increases linearly with increasing distance between the figures, given a constant eye-scanning speed. This is a consequence of

$$(10) \qquad T = \frac{D}{V}$$

where 'T', 'D', and 'V' represent real time of scanning, real distance between objects scanned, and mean velocity of scanning, respectively. Hence, given (9) as our hypothesis and (9a) as its analysis, we should expect the result we get, namely that, for the typical participant in the map-scanning experiment, the time it takes for him to respond to the request to shift his focus from one figure in his image to another increases linearly with increasing distance between the figures.

As to the pictorialist's claim that mental images can have parts of which their subjects are unconscious, again the adverbialist need not necessarily disagree. The assertion

(11) The subjects in the experiment have mental images of the map without being conscious of all their parts.

can be analyzed (in first approximation) as

(11a) The subjects in the experiment undergo experiences much like those they would undergo were they to see the map, but it is not the case that they are conscious of all the qualitative features of these experiences.

(11a) expresses a hypothesis that explains the data the pictorialist appeals to, but it remains neutral between pictorialism and adverbialism.

I propose finally to examine one of the pieces of data which have been held to provide evidence for descriptionalism. What I shall try to show is that the adverbialist can explain this datum. The datum I am speaking of is the fact that not every feature of a mental image need be determinate. For example, a person can have an image of a speckled hen or a striped tiger without his image having a determinate number of speckles or stripes. This fact underpins certain experiments, the results of which have been taken by descriptionalists to count against pictorialism and for their view.[18] The experiments themselves need not concern us here. The claim I want to

[18] See, for example, Pylyshyn, "Imagery and Artificial Intelligence," *op. cit.*

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address is that only descriptionalism can provide a satisfactory explanation of the lack of determinacy in some mental images.

Let us begin by reminding ourselves of the descriptionalist's explanation. According to descriptionalism, images represent in the manner of language and not in the manner of pictures. Thus, a mental image of a striped tiger is, in important respects, like a string of words describing a striped tiger. Obviously this description may leave out various details, one of which may be the number of stripes, just as a description of the man I saw at the door may fail to mention whether he wore gloves. The hypothesis put forward, then, is that the indeterminacy of some mental-image features results from the linguistic or descriptive character of images.

Descriptionalists have argued that pictorialists cannot account for image indeterminacy, since a picture of a speckled hen or a striped tiger, for example, *must* have a determinate number of speckles or stripes. Let us see what explanation the adverbialist can offer.

Consider the statement:

(12) I have an image of a striped tiger.

According to adverbialism, (12) is equivalent to

(12a) I image a-striped-tiger-ly.

In (12a) 'a-striped-tiger-ly' stands for a compound mode of imaging whose character may be indirectly specified by saying that it is the mode that is typically present in the imagings of normal perceivers as a result of their viewing striped tigers (or pictures of such) in standard conditions. In these typical cases it is the fact that the viewed tigers or tiger pictures have stripes (regardless of variations in the number of stripes) which causes it to be the case that the mode, a-striped-tiger-ly, is present in experience. Now, given (12a), there really is no mental image that I have when I imagine a striped tiger. Hence the question "How many stripes are there on the image?" cannot be raised. Furthermore, the imaging mode, a-striped-tiger-ly, is not the same as the mode, a-tiger-with-N-stripes-ly, where N is some definite number. The conclusion I reach, then, is that image indeterminacy presents no problems for adverbialism.

I have argued in this section that a representative sample of the empirical data does not support either pictorialism or descriptionalism over adverbialism. But I have not yet explained why I prefer adverbialism. Nor have I addressed the reasons that are not experimentally based for (and against) the view that mental images exist. It is to these tasks that I turn in the final section.

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III

The claim that there are mental images, regardless of whether they are ultimately classifiable as physical entities, may be supported in several different ways. To begin with, it may be suggested that it is just *obvious* from inspection of the experiences we undergo in imagery that mental images exist. It seems to me that this appeal has little force. Phenomenological inspection of an image experience will no more tell us whether there really is an image than will inspection of a person's smile tell us whether there really is a smile, a smiling, or merely a person who smiles.

Another reason for postulating mental images—and historically a very important one—is the desire to account for the facts of hallucination and illusion: since a person can have an image experience of a pink rat, say, even though there are no real pink rats, it is often inferred that he must be related through his experience to a pink, rat-shaped image.

This appeal again has little force, since it should be clear that the adverbial approach to imagery can also account for the facts of hallucination and illusion. If, for example, I am correctly described as having an image of a pink rat, then 'pink' and 'rat' in this description are taken upon analysis to function as adverbs that express modes of my imaging. Hence my having the image does not require that there be any pink rats—it suffices that I image a-pink-rat-ly.

A third reason—and the last I shall mention—is that mental images are needed in order to assign plausible logical forms to the statements we make about image experiences. This reason strikes me as no more compelling than the previous ones. In my view, the adverbial theory provides us with a satisfactory alternative analysis of the logical forms of the relevant statements. Still, some philosophers might argue that the adverbial analysis of logical forms is inferior, since it is a good deal more complicated. But I am unpersuaded by this argument. Questions of simplicity for image discourse cannot be settled in isolation from the analysis of other discourse (for example, the analysis of statements, such as "Jones almost sang," which contain adverbs having nothing to do with imagery). Simplicity, in any event, has a metaphysical as well as a logical dimension. And metaphysically, at the level of concreta, it is clear that the adverbial approach wins out at least over the traditional view that mental images exist as nonphysical particulars. With respect to the more recent view of pictorialists and descriptionalists which has it that mental images are neural entities, the situation is not so clearcut. But even here I believe a case can be made for the adverbial view. Let me explain.

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According to pictorialists and descriptionalists, there are concrete neural processes (rotating, scanning, reading, for example) which act upon mental images. If, conforming to frequent recent practice, we use the term 'event' in a broad sense to include not merely changes but also state-tokens, processes, and other like concreta, then we may say that pictorialists and descriptionalists, in taking this view, thereby commit themselves to events.

The adverbialist of my stripe is not committed to any such entities. In my view, causal statements are best understood as involving a non-truth-functional connective: *the fact that ___ causes it to be the case that . . .*, rather than a relational causal predicate. Given this analysis, causal statements do not really denote events or quantify over them. Furthermore, it is not necessary to introduce events for any other purposes.[19]

Admittedly, my adverbial theory *does* introduce certain abstracta, namely properties and functions. But, arguably, entities of these types are needed in other contexts anyway (the former, for example, in the semantics of abstract singular terms and the latter in the semantics of everyday adverbs).

I claim, then, that there are no strong reasons for preferring approaches to mental imagery which postulate images over my adverbial approach. Are there positive reasons for preferring the latter approach to the former? I believe that there are.

Consider first the traditional view of mental images as nonphysical entities somehow located in private "spaces." It is well known that this view gives rise to all sorts of puzzling questions. For example, can images exist un-had? Can two persons experience numerically identical images? Do images have "rear" surfaces which aren't experienced? What are images made of? What is the connection between private image space and real space? Are there laws for private space? The adverbial theory of imagery has the advantage of avoiding these questions.

Turning to the more recent view that mental images are pictorial or descriptional representations located in the brain and redescribable in neural vocabulary, it must be admitted that some of the questions that confronted the traditional view either do not arise or can be answered. Still there remain a number of puzzling questions. For example, is it a contingent fact that mental images have physical redescriptions? If not, why not? (Is dualism generally a

[19] For arguments in defense of this sweeping claim, see Terence Horgan, "The Case against Events," *Philosophical Review*, LXXXVII, 1 (January 1978): 28–47. For arguments which lead to the rejection of mental events, see my "Against the Token Identity Theory" (with Horgan), forthcoming.

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necessarily false hypothesis?) If it is a contingent fact, then don't all the traditional difficulties reappear counterfactually with respect to the possible worlds in which mental images are not physical? If I blink my eyes while having an afterimage, do I undergo two different images or only one interrupted one? Some mental images seem to fade away, to have "fuzzy" endings, as it were. Do the neural entities with which mental images are to be identified themselves have "fuzzy" endings? If so, then, with respect to any given "fuzzy" mental image, is it at all plausible to suppose that there is a *single* "fuzzy" neural entity more suited for identification with that image than any other?[20] The adverbial theory again sidesteps these questions.

I conclude that the adverbial theory is not only well motivated but also defensible against both empirical and theoretical objections. Moreover, it has implications for our understanding of a wide variety of topics. For these reasons, it seems to me that the adverbial theory is an important theory which merits much further discussion.

MICHAEL TYE

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MENTAL IMAGES AND THEIR EXPLANATIONS*

IN "The Debate about Mental Imagery", Michael Tye challenges the claims of pictorialism and descriptionalism (henceforth "P & D") but not quite in the way he intends. In advocating adverbialism over P & D, Tye assumes the three theories to be competing theories of the same kind. This is a mistake. P & D are empirical hypotheses offered in the context of the research program of cognitive psychology, a program which attempts to explain our various cognitive capacities by assuming that the mind is both a representational and a computational system. Within this framework, P & D are not even themselves entire *theories* of our capacity to image. Rather they are rival claims about the specific nature of

[20] For further problems here, see Tye and Horgan, *op cit.*

*Abstract of a paper to be presented in an APA symposium on The Debate about Mental Imagery, December 29, 1984, commenting on Michael Tye's paper of the same title; see this JOURNAL, this issue, 678–691.

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the representations underlying our capacity to image, claims which belong to such larger theories of imaging. Adverbialism, in contrast, is a recommendation about how to analyze the *ordinary* way we talk about our mental life so as to avoid ontological commitment to what are taken to be undesirable mental objects (such as mental images). Even at the level of ontology, the three positions are not making rival claims. Adverbialism wants to deny that our experience of imaging involves the having of phenomenal images. P & D say nothing about phenomenal images; they claim, instead, that associated with the experience of imaging there are *data structures* of certain specified sorts.

Tye's principal concern—whether or not there are phenomenal images—is not one which P & D address. However, exercising only minor interpretative liberties, we can discover in Tye's paper an even more serious challenge to P & D. It is this: that information-processing theories of our capacity to image have no greater explanatory force than explanations available in common-sense psychology and, hence, are unnecessary. The fact that Tye offers us two kinds of alternative explanations—adverbial and "topic neutral"—both of which are in the common-sense idiom, supports this reading of his paper.

I shall argue that, in providing his alternative explanations, Tye has not succeeded in defending this claim. The important points are these. (1) In being part of an empirical research program, P & D are embedded in a whole system of explanations. To provide alternative explanations for two isolated experiments thus carries very little weight. (2) The two experiments Tye considers were, in some sense, designed to show that "the characteristics of an image evident in the experience of imagery did in fact index characteristics of the underlying data-structure."[1] It is thus not surprising that there is a common-sense explanation parallel to the information-processing one for these experiments. Other experiments, however, such as those designed to provide information about how we store and generate images, will not yield as readily to common-sense psychological explanations. (3) The specific adverbial explanations which Tye provides are much too sketchy to be convincing. To make his case, Tye needs to demonstrate how the adverbial mode can handle more complex image descriptions and, especially, *generalizations* over image experiences which advert to imaged objects and their properties. (4) The topic-neutral explanations have a certain appeal. However, they remain ultimately less explanatory than

[1] S. M. Kossly, *Image and Mind* (Cambridge, Mass.: Harvard, 1980), p. 30.

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those offered by P & D. They cannot explain, for example, why an experience that is not under the control of an external stimulus has the temporal characteristics of one that is.

BARBARA VON ECKARDT

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PERCEIVED WORLDS, INFERRED WORLDS, *THE* WORLD*

TWO major themes structure Professor Heelan's interesting and highly ambitious book. First, there is an attempt to characterize the "geometry" of the space of immediate awareness and to argue that this space was primordially, in our "untutored" state, describable by a hyperbolic geometry, only to be transformed into a perceptual space describable by Euclidean geometry under the impact of a transformation of our life into one enmeshed in "carpentered" environments. Although I am dubious that Heelan's data—from fascinating psychophysical experimentation, from the existence of certain illusions, from art, etc.—supports this grand historical claim about the space of perception or even that one can describe the world of "how things appear" in any single, consistent geometry of the sort appropriate for describing the physical world, I will not pursue this theme of his book except in passing.

I want to focus rather on Heelan's second major connected set of topics. *Space-perception and the Philosophy of Science*† offers an extended and highly imaginative attempt to outline an over-all approach to perception and natural science that will do better justice than do the usual accounts to the perplexing problem of reconciling the world as it manifestly is presented to us in perception and the world as our physical theory describes it to us. Based on an extended and sometimes metaphorical use of categories familiar to us from the hermeneutic phenomenologies of Heidegger and Merleau-Ponty, Heelan's account tries to point the way toward a resolution of the most distressing and frustrating difficulties of more familiar

*To be presented in an APA symposium on Patrick A. Heelan's *Space-perception and the Philosophy of Science*, December 29, 1984. Professor Heelan will comment; see this JOURNAL, this issue, 707/8.

†Berkeley: University of California Press, 1983.

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ways of assimilating into a coherent whole the worlds of immediate experience and of physics. But can his account really handle the familiar problems any better than the older approaches?

Let me first express my agreement with Heelan that none of the traditional resolutions of the problem seems to be fully satisfying. I agree also that the difficulties of the standard approaches can be seen most clearly when we consider the nature of space and our awareness of it, although I would add that the difficulties seem even more perplexing when we reflect on the contrast between physical and perceived time.

"Idealist" solutions of many varieties promote the primacy of perception in all its aspects (semantic, epistemic, and ontological). Theories of the nonperceived parts of the world become either mere shorthand for complex and general statements about the observable, or, at best, fictionalistic instrumentalistic devices for getting us around in the world of the immediately perceived by means of a spurious ontological commitment to theoretically inferred entities which, not being in the real ontology of the immediately perceivable, are reconstrued as mere "intervening variables." But it is dismaying indeed to be told that we ought to take molecules, atoms, electrons, and quarks, electromagnetic fields or curved space-time not as the genuine furniture of the world but merely as falsely hypostatized entities introduced as items of reference for a language which, when properly understood, is seen to be nonreferential in nature.

On the other hand, many "naturalistic" solutions, even when subtly and carefully devised, seem to fail to do justice to the genuine primacy of the immediately perceived. Contemporary naturalists try a variety of ways of allowing us to reconstrue our epistemic access to the world and our grasp of meaning in naturalistic terms in order to avoid the perplexity of a view that seems to eliminate the immediately perceived or else retain it as a trivial and insulated epiphenomenon, while retaining the full naturalistic world whose very existence we inferred initially from the regularities in the world of immediate perception and which is such that the very concepts we use to describe it had their apparent origin in some sort of association of linguistic features with items of immediate awareness. Even the subtlest of nonmentalist accounts of meaning or of the possibility of reconstruing our epistemic access to the natural world naturalistically despite its origins in an initial leap from the manifest to the natural world order, simply don't seem to do justice to the ineluctable "first-person" perspectival nature of our grasp of meaning and of the legitimation of belief. The mani-

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fold ways in which underdetermination keeps re-arising as a problem for all naturalistic theories, whether of the physical world or of the world of meanings or of psychological states of others, is a clear symptom of the dangers that ensue when the inferred naturalistic world is treated as though it were a given and the semantic and epistemic critique of its origins in our conceptual scheme is slighted.

I

Heelan's solution to the problem of reconciling the primacy of perception with the reality of the world as described to us by naturalistic science works, basically, by emphasizing the plasticity and extendibility of the world of the directly perceived. Not only is the world of which we are directly aware primary for Heelan in the semantic and epistemic sense, as the source of our meaning and legitimation of belief; it is the *ontological* ground of the world as well. But this world of which we are immediately aware is no fixed, highly restricted, and diminished world of immediate perception, to be contrasted with the wider and richer world inferred by science. The inferred entities of natural science are legitimated, for Heelan, by the fact that *they* constitute elements in an *extended* world of the immediately perceived.

The world we perceive is not the world of the phenomenalist's private red patches stripped of the full richness of ordinary physical objects. It is a *Lebenswelt* of intersubjective objects with infinitely extensible features. We perceive what we do only because we participate in a social form of life with a shared and communicable language. It is this embedding in a social and linguistic matrix which provides us with the full panoply of anticipations and expectations that are part and parcel of the world of our immediate perception. Of course our world of immediate experience also includes the phantasms of dreams, illusions, and hallucinations, but our ability to go on in the perceived world and to connect into a whole the various items of experience allows us to distinguish within our perceived world the mere idiosyncratic items of private delusory experience from the objective items of the world which we immediately perceive but which the others of our community immediately perceive as well.

Nor is the world of immediate experience a fixed body of elements, unchanged and unchangeable no matter how our knowledge, our social context, or our scientific beliefs change. Experience comes in "horizons," whole structures which provide the context of more particular experiences. And, under the impact of linguistic, scientific, or social change, whole horizons can change, in a

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manner which bears some relation to familiar perceptual *Gestalt* switches, but in a subtler, more continuous, and more profound way. It is Heelan's view that at one time our spatial "horizon," the very matrix into which all our spatial immediate perceptions fitted, was describable by a hyperbolic geometry. Only under the influence of scientific measurement processes (transported rigid rods, *et al.*) and the change of our environment from nature to the "carpentered" world of technical artifacts, has our spatial horizon slowly changed to that of the current prevailing Euclidean perceptual space.

Actually, even incompatible horizons can be available to a single observer, again in a manner that bears at least an analogy to the multiple appearances we can flip between in the familiar cases of figure-ground, etc. Single observers can, in different contexts of their lives, for example, have their appearances in the horizon appropriate to the psychologist's world of mere visual images or to the ordinary world of seen intersubjective objects; or their experience can be that which is available only to the specially trained scientist whose horizon can include in his perceptual world the most recherché items of theoretical inference. Yet, by a process of synthesis, we can overcome the incompatibility of horizons by moving to higher levels in which what was incompatible becomes integrated. As we shall see, this possibility plays a crucial role for Heelan in resolving notorious problems of the incompatibility of manifest and theoretically inferred worlds.

Most important for our purposes is Heelan's notion of extending the world of immediate perception by means of "readable technologies." For him the role of measuring instruments is crucial in science—a view shared, naturally, with anyone of even minimalist empiricist views about the introduction of theoretical concepts and the legitimation of theoretical belief in science. But for Heelan the most important role of measuring instruments is that, for the scientist appropriately trained in the theory that grounds their use, they can actually *extend* that scientist's world of the immediately perceived. Although the scientist might, at first, *infer* the existence, say, of a hyperon of strangeness +1 by seeing the streak of light in the spark chamber, knowing as he does the appropriate meaning of such streaks of light in such apparati in such contexts, he eventually comes to *see* the hyperon (and its strangeness?) itself. For, just as the visual text on a page becomes "invisible" to us as we read and directly grasp meanings and just as the neuronal processes in our nerves are invisible to us although their existence is causally essential for our having any perceptual experience at all, so the

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trained scientist eventually loses his conscious awareness of the streak of light in the spark chamber altogether, directly seeing the particle he once inferred from seeing the streak of light. "Look," he says, "there goes another one of those strangeness +1 hyperons." The "look" here, Heelan tells us, is to be taken in as literal a sense as it ever has. What was originally an item of the perceived world, the visible spark in the chamber, has become invisible, a transparent "window" into the world of subatomic particles. And what was originally an inferred but not perceived element of the world, the hyperon (and its strangeness?) has become a perceived item (and property?) in a *perceptual* world of subatomic particles and their features.

This extendibility of the world of the immediately perceived is absolutely crucial for Heelan. Though Heelan rarely uses the semantic mode of discourse, I think it safe to say that for him terms would not properly be meaningful unless they could be associated with items of a perceived world. Nor could any of our beliefs be genuinely legitimated unless they were tested by direct comparison with experience and found to hold in the world available to us through perception. Finally, and most radically, Heelan makes it clear that, in his view of ontology, to be is to be an item of an experienced world. If we were not able to extend our perceived world, by means of readable technologies, to encompass the items that science tells us constitute, in depth, the furniture of the world, then we could not properly understand the very concepts in which these "theoretical" items are described to us. We could have no legitimate grounds whatever for taking the propositions of theoretical science to be correct. Nor could the items of theoretical science be said to exist. Without the extendibility of the perceptual world which allows it to encompass the theoretical world of science, we would be reduced once again to some kind of instrumentalist or fictionalist view of the role of theoretical language in science, once again taking as seriously referential only that fragment of language (the "observation language") which talked about the genuine items of the world, those which we could encounter in our *Lebenswelt*.

Such a rich and subtle doctrine obviously outruns the possibility of a brief critique. But I shall try to ask some pertinent questions of three sorts concerning Heelan's general doctrines. First, is there really any legitimate way of extending the notion of "perceivable" to encompass the theoretical, as Heelan suggests, which doesn't do a great deal of injustice to the phenomenological notion of perceivable with which he is operating? Second, can the traditionally puzzling conflicts between the world as perceived and the world as

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described by science really be dissolved by Heelan's notion of the synthesis of conflicting perceptual horizons? Finally, can Heelan really plausibly grant to perceivability the semantic, epistemic, or ontological importance he credits to it, when the distinction between the perceived and the merely inferred is made in the way he wishes to make it?

II

Looking at the spark in the spark chamber, I see, perhaps, a flash of light between two grey metal plates. Or, being a little bit sophisticated scientifically, I see, rather, a spark jump between two electrified plates. But, according to Heelan, for the sophisticated physicist these possible perceptual contents vanish from view. In his perceptual horizon, extended by the readable technology of the spark chamber, there appears no flash of light, not even an electric spark. Instead what he *sees* is the passage of a hyperon carrying strangeness +1. But will this do? Better, will an account of perceptual worlds of this sort consort well with even the most residual and uneliminable fragment of the notion of perception which we must retain if we are to have a notion of perception plausibly in keeping with the phenomenological tradition?

Certainly from certain naturalistic perspectives such a way of talking about perception could be legitimated. From the naturalistic point of view there is just that long chain of causation, running from the strange hyperon, through the ionized gas in the chamber, thence to the emitted light, thence to the stimulated retinal cell, thence to the ionic current in the optical neural pathway, etc., finally ending up, modulated by the complex of causes residing in the receptive brain of the physicist, in his vocal exclamation. And if he says, "Look, a hyperon moved between the plates," why not describe him as perceiving the hyperon whose presence he reports, rather than the spark, which, though causally in the chain from hyperon to exclamation, is beneath his notice in this context?

But do we see the hyperon or its strangeness in any sense of "immediate perceptual awareness" that makes the first-person account of phenomenological experience the plausible account of a portion of the world which in fact it is? I doubt it. The plasticity of perception is much emphasized nowadays, but it goes only so far. When we see the drawing as that of a duck and someone says, "But don't you see the rabbit?", what we *see*, in the most direct sense possible, may indeed change. And the same is true in the usual *Gestalt* cases of figure-ground switch, etc. Perhaps it is even true to say that the surgical student *sees* differently once, in the mass of bloody innards before him, the pancreas is pointed out by the in-

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structor. But an extension of such genuine changes in perceptual awareness to the case in question is, I think, unwarranted. This is hard to show directly, but let me offer a few reasons for thinking it so.

I think that within the first-person perspective we need a notion of perceiving one thing *by* perceiving another and that this notion is not the same as the notion of perceiving something by means of a process that involves the other as causal intermediary. A neural pulse in my optic nerve is intermediate between any physical object seen and my perception of it, but I don't see tables by seeing my neural pulse. But when I "see" the hyperon I see it *by* seeing the spark, which I see by seeing the flash of light, which I see, perhaps, by seeing that immediate content of visual awareness so precious to empiricists and phenomenologists and so doubted by naturalistic lovers of illimitable plasticity of perception. Perhaps this "pure content of awareness" is "always already" infected by that theoretical structuring of even the most immediate which is emphasized by everyone from Whorf through Heidegger to Goodman. Perhaps it is plastic in the sense that, in one and the same physical-object situation, what I am perceptually aware of can vary with "horizonal" expectation, as it does when duck switches to rabbit and then back to duck. But that doesn't mean that there isn't some coherent sense of that of which I am perceptually aware, which is to be distinguished from all those items in ever more remote causal connections which, naturalistically speaking, are causally responsible for my awareness' being the way it is.

When we ask the physicist what he saw, and he tells us it was a strange hyperon, we ought not to stop there. For he will understand perfectly well what we mean when we continue the interrogation by asking not what he inferred to but what he *saw*. And I dare say we can get him to the point of admitting that what he really was perceptually aware of was a light flash against a visual background of greys. And I don't think that in so admitting this he is merely being misled by bad perceptual philosophy. Nor do I think that the fact that he didn't even notice this visual appearance when he was aware of it is devastating to the claim that, when push comes to shove, it is that of which he was really perceptually aware. At least I don't think that we can give up this way of describing things and have any first-person phenomenological way of viewing the matter worth holding onto against the claims of the eliminativist naturalist.

Let me try to back up this bald-faced claim with two somewhat indirect arguments.

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One doubt that I have about the plausibility of extending the notion of perception in the way Heelan intends rests upon a distinction between two kinds of concepts that appear in our theoretical discourse. On the one hand there are those concepts which, when applying at the theoretical level, *seem* at least, to have their origin in a transfer to that level of terms whose meaning originally accrues to them from the observational context. Thus we talk of the size and shape of a molecule (or even of an atomic nucleus) as if (despite frequent warnings to us) we meant the same thing by size and shape in this context as we do when talking of stones or blocks of wood. On the other hand there are those concepts (electric charge, strangeness) introduced totally *ab novo* in the theoretical context, terms invented by the scientist to stand for theoretical properties and relations of a sort totally unfamiliar to us in our ordinary perceptual experience of macroscopic objects.

Consider now an application of Heelan's notion of perception extended by a readable technology. There is no doubt that we are tempted to say that we *see* the bacillus when we look through the microscope and that we see the DNA molecule when we look at the phosphor screen of the electron microscope, although we are less so tempted when what we see are the printed reproductions of photographs of the microscope images. When we are dealing with those properties of the normally invisible which become perceptible to us by the extension of our perceptual capacities mediated by technology there is indeed a strong temptation to talk as though we have simply enlarged the realm of the immediately perceivable.

But what would count as *perceiving* the strangeness of a hyperon? With which mode of perception would we perceive it? Would we see it? Smell it? Hear it? Now I don't want to push too far this distinction between those properties of the normally unperceptible which are "analogous" to those normally perceptible and those which bear no such analogy. But it does seem to me that reflection on the implausibility of taking as perceivable, even in a sense like Heelan's which allows for "direct" perception by means of the extendibility of our perceptual faculties by means of technology, of such properties as the charge or strangeness of a particle, will push us in a certain familiar direction. It is hard to resist the pressure of intuition, suggested by reflection on cases like these, that the repertory of features available to us by means of perception, no matter how loosely construed, is a fixed and determinate class. We can see geometric features and color, hear pitch and loudness, taste sweet and sour, etc. But no matter how extendible we may take perception to be by means of technology, we can never

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so extend it as to take into its range those theoretical features of the world whose very nature is outside this fixed and determinate class. If we are, however, to take the theoretical postulation of science as genuinely referential, taking it to be the case that if our best available theory to date requires us to postulate of elementary particles such features as strangeness, then we will be forced to deny Heelan's claim that even the utmost theoretically inferred reaches of natural being are, at least in principle, introducible into our perceptual world by means of a readable technology.

My other indirect argument against the extendibility of perception, as Heelan needs it for his case, is related to the so-called "problem of the underdetermination of theory by observation." In the philosophy of space and time observables are taken to be local relations among material objects, leaving the global physical geometry allegedly underdetermined by the totality of possible observational data. In philosophy of language and of psychology it is overt behavior which provides the allegedly exhaustive data base, and it is the attribution of meaning or of belief and desire which is alleged to be underdetermined by the totality of possible observational data. Now if perception reaches as far as theoretical inference, then it is hard to see what sense to make of these underdetermination claims.

One obvious response is to be delighted with this result, since it would undercut some profound skeptical problems in one easy dismissal. But I think that would be too hasty a reply. Imagine two scientists, espousing alternative, incompatible theoretical accounts of the world, where the two accounts agree on all possible observational facts in the older, non-Heelan, sense. Each, by means of a readable technology, extends his realm of the perceivable to the limits of what he has theoretically inferred. Then each *perceives* a world which is contradictory to the world perceived by the other. Note that we can't dismiss one or both of these perceived worlds as delusive, like an illusion or hallucination or a rejected theory, since we are taking it that both theories are fully "observationally adequate" in the older sense of that phrase.

Now Heelan is rather permissive about worlds, tolerating as many of them as there are different "horizons" of perception. But here things seem to have got out of hand. Previously we had the problem of incompatible assertions seemingly equally compatible with all the observational data that served, ultimately, to epistemically adjudicate them. The older responses were familiar: take a reductionist meaning theory, which made the assertions equivalent; offer a differential confirmation, which made one theory more be-

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lievable than the other despite their observational equivalence, etc. But here we seem to be stuck with a version of what has always been, at least to me, one of the least satisfying ways out of the underdetermination dilemma. This is the "way out" that allows for a soft notion of truth and a permissive notion of rationality, so that the exponents of both theories are taken to be equally correct. Of course all notions of rationality and truth are now "internalized" to avoid the obvious problems of tolerating a "theory-independent" contradictory picture of the world. But in Heelan's picture this permissiveness is extended, in what is surely a disturbing way, to what we perceive; so one cosmologist is perfectly within his rights (and, indeed, correct) when he says "Space-time is flat, and I know it because I simply *see* that it is," and the other is equally justified (and correct) when he says, "Space-time isn't flat, and I know it because I *see* that it is curved." Heelan's flexibility in allowing perception to reach to the limits of theoretical inference is going to make the very world of what is immediately perceptually available to us as infected with underdetermination as our most open and indeterminate theoretical inference once was.

I also think that this breaking down of the perceivable/nonperceivable distinction, in a manner as radical as the one Heelan proposes, also makes for difficulties in reconstructing important episodes in the history of contemporary science, such as the legitimation of special relativity, as against the aether theories, and Schrödinger's claim to have proved the *equivalence* of his version of quantum mechanics with Heisenberg's. These require that we take quite seriously the claim that whatever constitutes the bounds on what is directly perceivable, simultaneity of distant events and the wave function and its time variation (or nonvariation) are outside those bounds.

III

The questions raised above relate to Heelan's liberality in extending the realm of the perceivable to the full range of theoretically inferred entities and properties. Here I would like, rather, to probe into his way of handling the notorious conflicts that arise when we compare the world as manifested to us in ordinary perception with the world inferred by our theoretical natural science.

The apparent inconsistency between worlds, equally perceptible on Heelan's view and all of which we wish to hold onto in our science, is puzzling. Consider, for example, the apparent incompatibility between the table as it appears to us in our manifest perceptual image and the scientist's molecularly constituted table. Unlike some, Heelan is not willing to argue for the compatibility of the

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two descriptions of one and the same object. Nor is he willing to accept the representative realist's solution of keeping only the scientist's table "out there," reducing the manifest table to a sensory image "in the mind" (or something of that sort).

His solution puzzles me, though. It is to argue that the two tables are constituents of two incompatible horizonal perceptual worlds. It is not as though we had to decide which table is out there in space; for there is not one space at all, but two distinct spatial worlds each populated by its own appropriate, manifest or scientific, objects. We have perceptual access to the manifest world with our eyes and to the scientific world with the extension of our perceptual apparatus by means of a readable technology, but these constitute alternative windows into distinct, incompatible worlds.

But will this do? We certainly need to hear a lot more before we can be satisfied with a solution of this kind. It is part of our normal understanding that there is but one table "out there" and that it is constituted of the small parts described to us by science. It is bad enough, in the representative realist's picture, to be told that the continuous, colored thing we see is not a table in physical space at all, but a private bit of awareness belonging only to the perceptual world of the individual perceiver. But is it any more satisfying to be told, as Heelan tells us, that there are two wholly distinct worlds of spatial things, each objective, each perceivable, but incompatible with each other and populated by wholly different ontologies of things? We normally take it that the physical table is *identical* with the structured collection of molecules that make it up. Are we to deny this? If so, what is to play the role in this picture of the world of the identity relation (or, perhaps better, the constitution relation) between a macro-object and its micro-constituent parts?

Of course this peculiar denial of identity occurs in the representative realist's picture as well, and perhaps, in just as perplexing a way. In both cases what we do presumably is replace the claim of identity with some alternative. In the case of the representative realist it is presumably some special causal relation that the scientific table bears to the sensation of a table which makes the latter a sensation of the former. But Heelan, with his distinct, incompatible total worlds won't have a causal relation (which is, I would suppose, intra-worldly) to rely on here. He does hint at solutions to this problem, but all seem counterintuitive at best. We are told that the two tables can be "methodologically connected" with each other, and they are described as "counterparts"; but just what kind of relation is the counterpart relation supposed to be? We are re-

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minded of options suggested by those who deal in the ontologies of alternative possible worlds, but we are hardly given an account that makes clear to us what is going on.

Now Heelan does present a "lattice" model of the structural interrelations among these various horizonal worlds. In particular, he suggests that, for two incompatible worlds like that of manifest and scientific objects, there will be a "logical sum" world that will be their synthesis. Presumably the manifest and scientific table would find their joint home in such a world. But when we see what goes on here we will still be puzzled. For this is really not a *synthesis* of the two worlds, but rather a disjunction of them. It is the world available to the perceiver who can *switch* from one horizonal mode of perceiving to another, in a manner similar to a *Gestalt* switch but distinguished from it by "a sense of continuity between the elements." But here there is still a multiplicity of worlds and hence a multiplicity of tables. Once again we are left with the question of the relation between the two tables. It is certainly not identity. It is some sort of methodological continuity. But the obvious solutions won't do. We can't say that the manifest and scientific tables are connected by being "in the same place"; for the very spaces in which they exist are distinct from each other. And if we try to use the "sense of continuity" from one perceptual world to the other to establish the relationship, we become puzzled as to just how distinct these worlds can be if they are "connectible" by a continuum. A continuum of what? Of continuously varying but distinct worlds? Of what sorts? What kind of continuity takes us from the world of visually extended colored patches to that of strange quarks? Here I think the claimed radical distinctness of perceptual horizonal worlds is coming into sharp conflict with the desire to claim them all as aspects of a single world multiply perceived.

IV

Finally, my third major question: Can Heelan really rest on perceivability, as he understands it, the full weight of semantic, epistemic, and ontological importance he wants it to bear? The possibility of an entity entering into a horizonal perceptual world is all-important for Heelan. Though he does not have much use for the semantic mode of discourse, it is consistent with everything he says to maintain that a concept is not intelligible to us unless it can be associated in some way with an element of perceptual experience, i.e., with some feature of a perceived world. He is explicit that it is being perceivable which legitimizes our belief in the existence of entities and features. Merely being able to infer to these things and properties would not be enough to warrant belief in

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their reality. Only insofar as the world of inferred scientific entities and features is a world that can be the object of our perception, extended if necessary by means of a readable technology, is it a world in which we ought to believe. The sole ground of legitimate belief is perception, and the limits of perceivability are the very limits of legitimate belief.

Even more crucially, ontology is (in some sense) an ontology of the perceived. Scientific entities exist only insofar as they exist to—and for—human perceivers. Only as elements of horizonal *Lebenswelten* do entities "exist" in the proper meaning of the term.

But is the distinction between what is perceived and what is inferred, as Heelan makes that distinction, profound enough to bear the weight he places on it? Consider, for example, a scientist experimentally pursuing the strange hyperon through the flashes it causes in the spark chamber. Initially he perceives (in Heelan's terms) the spark, only inferring, by his best available theory to date, the presence of the strange hyperon whose passage resulted in the ionization, which resulted in the voltage discharge, which resulted in the visible spark. Later, in the manner Heelan describes, the spark itself becomes, for this scientist, a "window," not itself present to his conscious awareness, through which the scientist "sees" the strange hyperon itself.

Before this perceptual shift did the scientist not know what 'is a strange hyperon' meant? Did he have no legitimate grounds for asserting the existence of the strange hyperon? Was it false to say that the hyperon existed and that its strangeness was predicable of it? Did intelligibility of concept, legitimacy of belief, existence of entity and feature change with the merely psychological change from (a) the deliberate inference of particle from observed visual image to (b) the immediate exclamation of "I just saw the strange hyperon we were looking for" without even noticing the flash of light? Surely this is implausible. It just doesn't seem that being perceived (versus being inferred) as Heelan makes that distinction is a sufficient distinction on which to hang so much semantic, epistemic, and ontological weight.

But perhaps it is not being perceived which is crucial but, rather, being *perceivable*. Still difficulties arise. Suppose it just turns out to be the case that we, as a matter of empirical psychological fact, can never "internalize" the relativistic quantum field theory of elementary particles to the point where we don't need first to make the observation on the level of "I see the spark," and only then infer "So I infer there is a strange hyperon present." Should we then reject the semantic legitimacy of our theoretical concepts, the epistemic warrant of our experimental data and theoretical infer-

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ence, and deny the ontology of our best available theories, merely because of the psychological inability to short-circuit the inferential process from observation in the way Heelan describes? Something seems peculiar here.

What disturbs me about Heelan's approach is his desire to maintain the rather radical view of the primacy of perception most congenial to the eliminationist or reductionist type of empiricist-phenomenologist while accepting the semantic, epistemic, and ontological tolerance of the inferential realist. I think that, for Heelan, concepts are intelligible only if they apply to the directly perceivable. I am sure that for him propositions are epistemically legitimate only if they can be supported by direct observational confirmation of their truth. And, for him, being is truly predicable only of that which is (or could be) in the realm of direct perception. But, to avoid the eliminationist or reductionist tendencies of such radically "primacy of perception" views, he wants to *extend* the notion of direct perception to the limits of the theoretically inferred.

Now, in connection with the first group of questions I posed, I argued that I thought this extension was incompatible with what 'directly perceivable' must be taken to mean if we are to take the empiricist-phenomenologist notion of primacy of perception at its full weight. Here I am arguing that the weak psychological distinction that Heelan espouses between being perceived and, instead, being merely inferred in the context of a holistic theory, is insufficient to support the radical semantic, epistemic, and ontological weight he places on the value of being perceived or perceivable. If the distinction between merely holistically *inferring* the existence of hyperons and strangeness and, therefore, regarding these concepts as not fully meaningful, the propositions about them as not genuinely true and believable, their existence as not seriously ontological, and, on the other hand, actually *perceiving* strange hyperons and, therefore, taking the concepts to be semantically fully genuine, the propositions about them fully true and believable, and their existence ontologically serious, is merely that of sliding from the status of observing the spark and inferring the strange hyperon to unconsciously going from the physical situation of perception to the consciously unmediated exclamation "I see the strange hyperon," it is hard to see why that rather soft psychological distinction should bear so much semantic, epistemic, or ontological weight—a weight more easily attributable when the distinction was the much more radical earlier one between a perceivable world and an inferred world forever barred to perceptual access.

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PERCEIVED WORLDS ARE INTERPRETED WORLDS*

Professor Lawrence Sklar's summary of the core epistemological and ontological positions presented in *Space Perception and the Philosophy of Science*[1] is careful and admirable. I thank him for this and for the good-natured style of his commentary.

The disagreement between us, however, is too profound to be adjudicated in the usual way, by an appeal to facts and common understandings, since the questions at issue concern what are facts, and whether our common understanding about how we use them, is correct.

Sklar, quite rightly, focuses on critical cases where he believes the plasticity of perception would be overextended by my account. For instance, he argues that the scientist, who says he "sees" a particle of strangeness +1 in the spark generated by a spark chamber, really sees only a "flash" of light from which he then infers the hyperon. I would conclude (on the basis of chapters 10 and 11) that the scientific research program in question is fulfilled when a skilled scientist embodied in the readable technology of the spark chamber would literally *perceive* the hyperon. Before the fulfilment of the program, though still only a gleam in the scientist's eye, the hyperon is nevertheless a meaningfully structured perceptual anticipation. Since a readable technology is a "detachable [sense] organ"[2], a new form of perceiving is constituted by the spark chamber, say, a *spark-chamber-viewing* which constitutes its own horizons.

The reason for this is the hermeneutical structure of perception.[3] Although direct perceptual recognition is not mediated by inductive or deductive inferences from prior knowns, there are, nevertheless, mediations of a hermeneutical or interpretative sort hidden from the first-person perceiver. From a third-person perspective, sensory organs and their extensions serve as signal-processing devices for the whole organism in the context of its *Lebenswelt*. The semantical space of the signal, however, is different from the semantical space of the object; they are two spaces connected by the hermeneutical (cf. C. S. Peirce's "abductive") structure of the perceptual act itself. Hermeneutics is both a method of inquiry and a

*Abstract of a paper to be presented in an APA symposium on Patrick Heelan's *Space-perception and the Philosophy of Science*, December 29, 1984, commenting on Lawrence Sklar's article, this JOURNAL, this issue, 693–706.

[1] Berkeley: U of California Press, 1983. References, unless otherwise stated, are to this book.

[2] Maurice Merleau-Ponty, "Eye and Mind," in *The Primacy of Perception* (Evanston, Ill.: Northwestern UP, 1964), p. 178.

[3] Cf. my "Perception as a Hermeneutical Act," *Review of Metaphysics*, XXXVII, 1 (September 1983): 61–75.

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structure of being[4]; as a fulfilled cognitive act, perception is structurally hermeneutic. This hermeneutic structure ensures that perceptual horizons are never unique and infallible. It is always possible to address the same perceptual field with new questions, and so to find new answers even in the old signs. Perceptual horizons then are historically changing; for the same reason, they are extensible by readable technologies.[5]

Side by side with simple sensory horizons of the world—the only ones recognized by Sklar—are other horizons made accessible by the use of readable technologies, for example, *thermometer-viewing* horizons, *ruler- and clock-viewing* horizons and, in general, *instrument-X-viewing* horizons, where *X* is some appropriate readable technology. All these multiple complementary viewpoints, *together with the perceivers who possess them,* constitute horizons of the *Lebenswelt* and, therefore, of reality. Though complementary, they are not contradictory, since they relate to different semantical spaces as appropriated by differently—incompatibly—embodied perceivers. My point of view is then naturalistic, pragmaticist, and pluralist; the knower does not "look into" the world from outside, the knower is part of the world—and a variable part at that! For example, besides primitive hyperbolic viewing, there are at least three forms of ruler-and-clock-viewing which give three different physical space/time horizons. As long as our perceptual interests lie within those satisfied by the technologies of classical physics, we have access to flat Euclidean horizons; but when these interests are not satisfied, we can use relativistic or quantum-mechanical ruler-and-clock technologies which give us different curved or "granular" space/time horizons. All are real, because all are equally perceptual horizons of real—though differently embodied—perceivers.

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[4] Cf. Martin Heidegger, *Being and Time,* J. Macquarrie and E. Robinson, trans. (New York: Harper & Row, 1962).

[5] Cf. ch. 13, and the writer's "Natural Science as a Hermeneutic of Instrumentation," *Philosophy of Science,* L, 2 (June 1983): 181–204. Such flexibility defines the *post-modernist* attitude; cf. Jean-Francois Lyotard, *The Post-modern Condition: A Report on Knowledge,* G. Bennington and B. Massumi, trans. (Minneapolis: U of Minnesota Press, 1983), pp. xxiii-xxv.

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DISTRIBUTIVE JUSTICE, CONTRACT, AND EQUALITY*

WHAT is the relationship between the contractual arrangements that underlie agreement-regulated cooperative enterprises and fair distribution of benefit and burden within such enterprises? Under what conditions, if any, would fair distribution be a simple function of the terms on which the participants have agreed to become involved? In exploring this question I shall move from cases of wholly unconstrained agreement through cases of mildly constrained agreement to cases where a great many conditions are imposed on the agreements which are held by would-be contractarians to be the source of fair distributive arrangements. In discussing what I call *multi-condition* contractarian theories—and these will provide the focus for much of the paper—I shall argue that the quest for normative adequacy, which is the driving force behind the view that only heavily constrained contractual arrangements can be expected to generate a fair distribution of benefits and burdens, is guided throughout by an unacknowledged and *contract-independent* principle of distributive justice, one that calls for imposition of equal burdens on participants in return for conferment on them of entitlement to equal benefit.

I

According to the most straightforward account of justice along contractarian lines, fair distribution within ongoing cooperative enterprises is a simple function of the terms on which participants have agreed to take part. Provided the actual distribution of benefits and burdens is in conformity with the terms of the agreement in which the enterprise had its origins, its fairness is assured: only if there is some lack of correspondence between the distribution actually effected by the ongoing enterprise and the division of benefit and burden envisaged in the pre-participation agreement are adversely affected parties in a position to complain of unfair treatment.

If deficiencies in a contractarian approach are to be explored which are independent of disputes about what is to count as agreement in particular contexts or about how the content and scope of particular agreements is to be determined, it is best to concentrate on the sort of case, untypical though it is, in which partic-

*To be presented in an APA symposium on Political Philosophy, December 29, 1984. Lawrence Becker will comment: see this JOURNAL, this issue, 719.

Work on this paper was begun during my tenure of a Fellowship at the Institute for Advanced Studies in the Humanities at the University of Edinburgh. Earlier versions have been read at the Atlantic Philosophy Conference in Cornerbrook, Newfoundland, at a Philosophy Colloquium at Queen's University, and at the Philosophy World Congress in Montreal.

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ipants can be said to have given *express consent* to the *over-all distribution* of benefits and burdens effected over time by an enterprise precisely because they have taken an active part in the negotiations that established and structured it. Whereas normally people are in a position to decide only whether or not to take part in some enterprise on terms that others have laid down, in the sort of situation envisaged here each of the prospective participants is in a position not only to decide whether or not to take part but also to help determine what the rules governing the enterprise shall be, including the rules that structure the roles of the various participants. Since situations of this rather unusual sort provide contractarians with ideally favorable circumstances for the application of their view of the ground of judgments about the fairness of distributive arrangements, they also furnish for critics a suitably demanding setting in which to put their objections to the test.

II

Suppose, then, that *A*, *B*, and *C* are parties to negotiations that are to give structure to some enterprise they have a shared interest in establishing. Is the fairness of the distribution of benefits and burdens within the enterprise once it is duly launched a simple function of its conformity to the terms of the pre-participation agreement concluded by *A*, *B*, and *C*?

To a contractarian theory of this rather crude sort, the conclusive objection is that a fair distribution cannot be relied upon to emerge from the process of negotiation unless (a) the parties have been able to reach agreement without resort to coercion and (b) they have had access to reasonably adequate information about the pros and cons of different ways of dividing up the benefits and burdens of participation. In the absence of fulfillment of what we may call the *no-coercion* and *adequate-information* conditions, the agreements that structure an enterprise cannot be expected to generate a fair distribution even if all the future participants have been involved in enterprise-structuring negotiations of the rather special sort described.

III

Are these two conditions, then, the only conditions that need to be insisted upon to ensure fair distribution within an agreement-regulated enterprise? Or are there cases of enterprises that effect an unfair distribution despite the fact that all participants have freely negotiated the terms of participation and done so, moreover, on the basis of adequate information about the implications of alternative schemes of cooperation?

Consider the following putative counterexamples to the claim—characteristic, let us suppose, of a second (somewhat less crude)

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contractarian theory—that the fairness of the distribution of benefits and burdens within an enterprise is assured if there is free and informed consent to that distribution on the part of all the future participants.

(1) First, there are cases where not all parties are equally competent to make use of the information available to them in the formation or implementation of suitable—interest-safeguarding—negotiating strategies. *A*, for example, may be a minor, of whose inexperience *B* and *C* take advantage. Or *A* may be deficient in intelligence, with *B* and *C* quite prepared to allow or encourage him to adopt plainly disadvantageous negotiating positions. In situations of these sorts meticulous compliance on the part of *B* and *C* with the requirement that they share all relevant information with *A* will not suffice to eliminate unfairness in the agreement-generated distribution of benefits and burdens: even casual examination of the terms of the agreement may reveal how stupidly *A* has made use of the information he had. Although it is chiefly blameless stupidity that is at issue in these contexts, not the stupidity associated with careless or sloppy use of information, it seems likely—though it needs to be argued—that the fairness of an agreement can be vitiated even by culpable stupidity.

(2) Secondly, there are cases where *A*'s pre-agreement resources—the resources on which he will have to fall back and with which he will have to make do if no deal at all is struck—may be meager by comparison with those over which *B* and *C* have command. In these circumstances *B* and *C* are in a position to drive a hard bargain. At least in cases in which *A*'s unenviable predicament is in no sense his own fault, the driving of a hard bargain by *B* and *C* will generate an unfair distribution of benefit and burden within the enterprise. Even where *A* is personally to blame for the poor bargaining position in which he finds himself, it ought not to be concluded too hastily—though the question is of course controversial—that no injustice would be done by the imposition of markedly unfavorable terms on *A*. There is, after all, something to be said for the view that each round of bargaining should be undertaken by the parties as though it were the first, with a determined effort by them to eliminate the effects, for good or ill, of past successes and failures.

(3) Thirdly, there are cases where the ability of the parties to contribute to the cooperative venture they are structuring may differ in ways that also contribute to inequality in their bargaining power. Thus it is not uncommon for people whose skills are in short supply (or whose skills happen to be highly regarded for

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some historically contingent reason) to demand—and to get—a better deal for themselves in agreements setting out the terms of cooperation in some enterprise. At least where differences in the ability of the parties to secure an advantageous niche for themselves are a function of factors outside their control—factors like natural talent, home circumstances, and opportunity to develop latent capacities—it is inconsistent with fair distribution of the benefits and burdens of social cooperation for those who happen to have socially valued or scarce skills to take advantage of their greater bargaining power.

(4) Finally, differences in the character traits of the parties may also be an obstacle to recognition of the fairness of the distribution effected by agreements freely concluded on the basis of adequate information about the implications of competing schemes. *A*, for example, may be generous-spirited, disinclined to drive a hard bargain, whereas *B* and *C* may be aggressive and selfish, quite prepared to extract whatever concessions they can. The fact that *A* may in a sense have himself to blame for his softness in the negotiations does not serve to show that the distribution effected by agreements in these circumstances is not unfair. A distinction must be drawn between the question whether the agreement concluded by *A*, *B*, and *C* is a fair agreement and the question of how responsibility (and blame) is to be divided among *A*, *B*, and *C* for any unfairness in the terms. It would not be at all paradoxical if *A* were found to be at least partly to blame for the fact that an agreement to which he was a voluntary signatory imposed unfairly heavy burdens on him or provided him with unfairly meager benefits.

IV

The natural response of the would-be contractarian to these counterexamples—should he be impressed by them—is to reformulate his position yet again, adding further conditions to the no-coercion and adequate-information conditions already in place. According to what might be called *multi-condition* contractarianism, all of a rather formidable set of additional conditions must be satisfied before any actual agreement about the terms of participation can be relied upon to yield a fair distribution of benefits and burdens within an enterprise. Thus the parties to the agreement (it may be held) must be supposed to be approximately equal in their capacity to make use of the information at their disposal in working out the advantages and disadvantages of competing schemes for the distribution of benefits and burdens. Again, their bargaining power must be approximately equal. Furthermore, they must be reasona-

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bly alike in their attitude toward the driving of a hard bargain. [It will be all right from the standpoint of this last condition if all of *A*, *B*, and *C* are disposed to take a "tough" (self-interset) line in such negotiations. And it will be all right also if they are all disposed, to about the same extent, to be solicitous of the interests of their partners-to-be. But if *B* and *C* are disposed to take a tough line, while *A* is unprepared to be comparably aggressive in the defense of his own interests, the terms of any agreement they conclude are most unlikely to generate a fair distribution of benefits and burdens.]

The adequacy of any such multi-condition contractarian theory depends critically on whether the conditions it incorporates can be elucidated and justified without appeal, direct or oblique, to principles of fairness. It is, after all, the hallmark of a truly contractarian theory of distributive justice that it seeks to *ground* the fairness of distributive arrangements in the fact of consent to them. It would consequently be self-defeating for any fairness condition to be attached to consent. To permit scrutiny of the fairness of the terms on which consent is given to some cooperative arrangement is tantamount to endorsement of a contract-independent conception of fairness.

Some of the conditions in question—those carried forward from the less elaborate version of the contractarian view which has already been found wanting—are easier to square with a thoroughgoing contractarian approach than the others. Thus the no-coercion condition (it can be argued) serves to underline the fact that *forced* agreement comes close to being no agreement at all. (Even if there should be some reluctance to make it self-contradictory to talk of "agreement" as issuing from negotiations in which there is express resort to coercion—and a parallel reluctance to represent "voluntary agreement" as a sheer tautology—even noncontractarians concede that only uncoerced agreements are morally binding.) Again, where the adequate-information condition is not fulfilled, there will be room for argument about the content of the agreement since there will be no assurance that the contracting parties are parties to *one and the same agreement.*

It is thus not implausible to hold that the first two conditions can be accounted for without inconsistency by the sponsor of a multi-condition contractarian theory. The no-coercion condition need occasion no embarrassment because when it is violated we do not have the sort of agreement that is even prima facie binding. And the adequate-information condition poses no serious problem

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because in situations in which it is not met we may be unable to say, without risk of challenge, what the terms of the agreement in fact are.

What about the remaining conditions? Do they pass muster in much the same way?

Appearances are, I think, misleading. It is true, for example, that these further conditions can be identified and applied without appeal to contract-independent principles of distributive justice. Thus it can be determined without any such appeal both what the *equal-astuteness* condition (as the first of these additional conditions might be called) amounts to and also whether it is satisfied in particular cases; and the same can be said of the other two conditions, the equal-bargaining-power condition and the condition that calls for the parties to have similar attitudes toward bargaining. But it does not follow that recognition of these conditions poses no threat to a strictly contractarian account of distributive justice. This threat can be brought out into the open in two ways: first, by inquiring into the rationale underlying insistence on these additional conditions and, second, by trying to ascertain how strong the correlation is between agreements that meet all these conditions and agreements that secure a fair distribution of benefit and burden within agreement-regulated enterprises.

V

(1) What, then, is the motive behind insistence on these conditions? The answer, it would seem, is closely tied to the recognition that actual agreements that do not satisfy them are likely to favor the parties who are more astute (or whose bargaining position is stronger or who are prepared to be ruthless in pursuit of their own interests) and *thus* be unfair to those who are less astute (or whose bargaining position is weaker or who are predisposed to be solicitous of the interests of others). Tacit appeal is thus being made to our sense of the fundamental unfairness of situations in which disadvantageous terms are imposed on people who happen to have less flair at the negotiating table or whose initial bargaining position happens to be weaker or who, for reasons of temperament or conviction, are unprepared to haggle. Yet no such appeal is permissible for the consistent contractarian, since it undermines the claim—crucial to his position—that judgments of fairness are *grounded* in judgments about actual contractual arrangements.

(2) But is there even an extensional equivalence between agreements that issue in fair distributive arrangements and agreements that satisfy all the conditions in a multi-condition theory? If the theory offered a wholly accurate picture of the constraints that

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must be imposed on the agreements people actually conclude with one another if these are to ensure fair distribution within the enterprises they regulate, it would have to be the case *both* (a) that agreements that violate any of these conditions do not yield a fair distribution of benefits and burdens among the parties, *and* (b) that agreements that satisfy these conditions can be relied upon to yield a fair distribution.

Yet both (a) and (b) are false.

(a) Take the case where *A* is a less astute negotiator than *B* and *C*. Are we bound to say, simply because the equal-astuteness condition is not satisfied, that any agrement they conclude will be unfair to *A*? Clearly not. For one thing, *A* may on this particular occasion succeed with an effort in safeguarding his interests: though not as shrewd as *B* and *C*, he may nevertheless turn in a good performance at the negotiating table. Alternatively, *B* and *C*, recognizing *A*'s deficiencies, may make some effort to look out for his interests and go out of their way not to take advantage of their greater negotiating skill. In either case, the terms of the agreement may well be fair—and the distribution effected by the agreement fair in its turn—the nonfulfillment of the equal-astuteness condition notwithstanding.

(b) Take the case now where the parties are equally astute or must be reckoned to be so on the basis of past performance. Does this ensure that the sort of unfair (agreement-determined) distribution against which this third condition is designed to offer protection will not be the upshot of the agreement concluded by the parties on a particular occasion? Again, clearly not. What if *A* should succeed in outwitting *B* and *C* by getting them to accept terms markedly more favorable to himself than to them, even though he is not in general better at negotiating advantageous arrangements than they are? Should we not say in these circumstances that the distribution effected by the agreement was unfair after all—unfair to *B* and *C* this time—despite fulfillment of the equal-astuteness condition?

The equal-astuteness condition, then, is open to objection not only because the rationale for its inclusion jeopardizes the claim—crucial to a strictly contractarian theory—that judgments about the fair division of benefits and burdens within cooperative enterprises are grounded in the fact of consent to that division on the part of participants but also because there is an insufficiently impressive correlation between agreements that satisfy the equal-astuteness condition and agreements that yield a fair distribution.

The two objections are, of course, related. Once it is recognized

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that the only plausible rationale for the equal astuteness condition is one that connects respect for this condition with elimination of the risk of agreements being concluded that favor the more astute of the contracting parties at the expense of the less astute, the question is bound to arise how reliably the risk can be eliminated by insistence on this condition. No doubt as a means of reducing the likelihood of agreements being made that are unfair because they favor the more astute, the condition has an obvious use. However, it articulates neither a necessary nor a sufficient condition of achievement of the objective desired—viz. elimination of the sort of substantive unfairness which consists in the securing of disproportionately favorable terms by clever negotiators. It is not a necessary condition because the more astute of the contracting parties may decide not to take advantage of their greater negotiating skill. And it is not a sufficient condition because the fact that the contracting parties are on the whole equally astute does not mean that they will perform equally well in all the negotiations they conduct. Whenever one of the parties performs either above or below his normal level, there is some risk of an unfair agreement being the upshot of negotiations.

Parallel objections can be urged against the remaining two conditions: the equal-bargaining-power condition and the condition that calls for the negotiating parties to have broadly similar attitudes toward hard bargaining. Although each of these can be identified and applied without appeal to contract-independent principles of distributive justice, appeal to such principles cannot be avoided both when it comes to explaining *why* these conditions are essential ingredients in a multi-condition theory and when it comes to the task of formulating them circumspectly in the (forlorn) endeavor to make them neither gratuitously restrictive nor unnecessarily permissive.

VI

What, then, is the principle of distributive fairness which furnishes the latent basis for insistence on the various constraints upon contractual arrangements embedded in the multi-condition theory? It is, I suggest, a principle of equality. A fair distribution imposes burdens of equal weight on all participants while giving them all an equal share in the benefits. From the vantagepoint of this sort of egalitarian principle, both the attractions and the deficiencies of a multi-condition contractarian theory come into focus.

Take the equal-astuteness condition. When it is not fulfilled, the cleverer negotiator will have an advantage in the discussions that set the terms for participation in an enterprise. If he has no scru-

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ples about using his superior skill to carve out for himself a more advantageous role within the enterprise than his dim-witted partners, the terms of participation are likely to be unfair, unfair because of his willingness to take advantage of the inferior negotiating skill of his partners. The mere fact that he has superior negotiating skill does not mean, of course, that the agreement that is the product of the enterprise-structuring negotiations will be unfair. Unfairness will result only if two further conditions are fulfilled: first, that he is disposed to use his superior negotiating skill to win for himself a better deal than his partners and, second, that he uses it successfully for this purpose on particular occasions. Nevertheless, an egalitarian principle of distributive fairness does provide a sort of rationale for the would-be contractarian's insistence on the equal-astuteness condition. When it is fulfilled, one kind of negotiating advantage—that connected with superior negotiating skill—is effectively neutralized, which in turn reduces the likelihood of any of the parties securing unfairly favorable terms for himself.

Again, a plausible account of insistence on the equal-bargaining condition can be provided from the vantagepoint of the hypothesis that fair agreements require participants to carry burdens of equal weight in, and to share equally in the proceeds of, any enterprise. When the parties to pre-participation negotiations are markedly unequal in bargaining power, those who enjoy superiority in this respect are in a position to dictate terms specially favorable to themselves. As with the equal-astuteness condition, of course, their greater leverage does not mean that the terms are *bound* to be unfair. A further condition is willingness, or determination, on their part to drive a hard bargain. Nevertheless, from a contractarian point of view, it makes sense to insist on some such condition, since when it is fulfilled the parties will not be able to drive a hard bargain even if they should be anxious to do so.

Consider, finally, the condition that calls for the parties to be alike in the attitude they take toward ruthlessness in the pursuit of self-interest in bargaining situations. Here too the condition, if fulfilled, will not ensure the fairness of the distribution arising out of the enterprise-structuring agreement. However similar the dispositions of the parties, inequalities in negotiating skill or in bargaining power may contribute to unfairness in the terms of the agreements they conclude. Nevertheless, fulfillment of this condition does appreciably diminish the likelihood of unfairness in these agreements. If none of the parties is predisposed by temperament or character to drive a hard bargain, differences in negotiating skill or

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in bargaining power will not be so likely to give rise to agreements unequally advantageous to the participants.

VII

The upshot of the argument I have sketched is that no strictly contractarian account of the fairness of the distribution effected under the terms of voluntary cooperative ventures can be defended. Sponsors of such an account face a dilemma. On the one hand, they may elect to restrict the conditions that must be imposed on agreements that are to be the source of fair distribution within agreement-regulated enterprises to those for which a plausible rationale can be provided without appeal to noncontractarian principles of distributive justice. This will enable them to accommodate the no-coercion and adequate-information conditions. But then many unfair distributive arrangements will have to be declared by the theory to be fair, simply because they are the upshot of agreements that do not breach these conditions. On the other hand, would-be contractarians may try to incorporate various additional conditions into their account in order not to flout too many of our considered judgments of fairness. However, their position will now be in danger of collapse into a noncontractarian one once the rationale for these additional conditions is investigated. The various conditions embedded in the multi-condition account are devices for closing the gap between our considered judgments about fair distribution within cooperative enterprises and the distribution that is the typical upshot of actual agreements in many ordinary situations, even those which involve the free and informed consent of participants. It is, however, an unavowed—and contract-independent—principle of fair distribution which provides the latent basis for these conditions: they are introduced precisely because their fulfillment makes it more likely, even if it cannot guarantee, that this principle will not be breached by the agreements people actually conclude with one another. But if an agreement can itself be vetted as fair or unfair by direct appeal to principles of distributive justice, fair distribution of benefits and burdens cannot unmisleadingly be represented as a function of consent to that distribution.

ALISTAIR M. MACLEOD

Queen's University, Ontario

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CONSTRAINTS ON CONTRACTS*

PROFESSOR Macleod argues that contract theorists, even in the cases most favorable to them, face a dilemma. On the one hand, they may choose to impose only minimal constraints on what counts as a binding agreement—constraints such as the absence of coercion and the availability of adequate information—constraints that are clearly tied, conceptually, to the very notion of an agreement. But then such theorists will, Macleod says, be forced to accept as contractually fair many distributions that are *un*fair. (The latter notion of fairness is left undefined.) On the other hand, contract theorists may choose to extend their list of constraints—to include, for example, the requirements that the parties be equally knowledgeable about things relevant to the agreement, be equally able to use their knowledge in evaluating various alternative agreements, be relatively equal in bargaining power, and "reasonably alike in their attitudes toward driving a hard bargain." But, once contract theorists do that, Macleod says, they will be using an "unavowed—and *contract-independent*—principle of fair distribution."

I propose to argue that an extended list of constraints on contracts can be defended in much the same way Macleod defends the minimal list. Whether there is anything in such a defense (or such an extended list) that embarrasses contract theory depends on how its theoretical project is defined. I shall sketch a version of contract theory that seems to me to be immune from such embarrassment.

LAWRENCE BECKER

Hollins College

*Abstract of a paper to be presented in an APA symposium on Political Philosophy, December 29, 1984, commenting on Alistair Macleod, "Distributive Justice, Contract, and Equality"; see this JOURNAL, this issue, 709–718.

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AMERICAN PHILOSOPHICAL ASSOCIATION
EASTERN DIVISION

Abstracts of Colloquium Papers to Be Read at the Eighty-first Annual Meeting*

THE PEPPER-CROCE THESIS AND DEWEY'S "IDEALIST" AESTHETICS

(IV-F: American Philosophy)

John Dewey's *Art as Experience* is a major work in aesthetics and lies at the center of Dewey's philosophy. Yet a number of critics, beginning with Stephen Pepper and Benedetto Croce, have leveled the charge that the book is fundamentally inconsistent with Dewey's pragmatism—is in fact more idealistic than pragmatic. After summarizing the main arguments of Pepper, Croce, and George H. Douglas, I show that these are all founded on that element of Dewey's philosophy which he calls the "pervasive qualitative whole." To determine whether Dewey's critics are justified in their assertions, one must examine this idea. And it proves upon examination not only to show that Dewey's aesthetics is consistent with his pragmatism, but also to link Dewey firmly to William James and C. S. Peirce. The concept of the pervasive qualitative whole is found in the *Essays in Experimental Logic* as well as in *Experience and Nature* and *Logic: The Theory of Inquiry*. Moreover, it can be shown that Dewey combined major insights of Peirce and James in this concept—James's doctrine of the "fringe" or "horizon" of consciousness and his theory of "felt" relations and Peirce's concept of Quality or "Firstness." Consequently, not only is Dewey's aesthetics internally consistent with his whole philosophy; it constitutes a brilliant synthesis of themes central to pragmatism. Pepper's, Croce's, and Douglas's criticisms are shown to be based upon partial or mistaken veiws of Dewey's philosophy.

THOMAS ALEXANDER

Emory University

MUSICAL KINDS

(V-E: Aesthetics)

The paper attempts to provide an ontology of musical works that accounts for two intuitions which seem to be in conflict: (1) that musical works are abstract entities not to be identified with their various scores and performances; and (2) that musical works are created. Nicholas Wolterstorff's argument[1] for the view that musical works "exist everlastingly" is examined

*Abstracts are here presented in alphabetical order, by author, with session number and colloquium title in parentheses, below the title of each paper. See the APA Program, this JOURNAL, LXXXI, 10 (October 1984): 625-639, for times and places.

[1] *Works and Worlds of Art* (New York: Oxford, 1980).

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and rejected. A resolution to the above conflict is found in Wolterstorff's own notion of a norm-kind. It is argued that musical works are norm-kinds, but, contrary to Wolterstorff, some norm-kinds, among them musical works, are brought into existence by way of human creative activity.

JAMES C. ANDERSON

University of Akron

MEANINGFUL WORK AND MARKET SOCIALISM

(II-E: Social Philosophy)

This paper considers the idea that in advanced industrial societies all citizens have a *right to meaningful work:* a right to opportunities for employment in which the work for which pay is received is interesting, calling for intelligence and initiative, and in which the worker has considerable freedom to determine how the work is to be done and a genuinely democratic say over the character of the work process and the policies pursued by the employing enterprise. I argue against acceptance of this right to meaningful work except insofar as portions of this right are incorporated in the core rights definitive of market socialism.

RICHARD J. ARNESON

University of California/San Diego

LOCKEAN PROVISO AND STATE-OF-NATURE THEORIES*

(IV-E: Political and Legal Philosophy)

The analysis of this paper is intended to bring out certain methodological flaws in some state-of-nature theories. It is organized around an examination of the place of the Lockean proviso in these theories. Section I concerns the relation of the Lockean proviso to non-pattern principles of justice; here I argue for the thesis that inclusion of a Lockean proviso renders a theory patterned, and so no non-pattern theory may include such a proviso. Section II concerns the tension between root ideas and the grounding of the Lockean proviso; here I argue that the fact that the Lockean proviso has grounding casts doubt on the depth of the root ideas. The more general point of section II is to mount an attack on the assumption that makes root ideas methodologically plausible: invariance of principles across changes of circumstance. Insofar as the critique of section II succeeds, the point has application to any state-of-nature theory and not merely to procedural or process theories.

J. HILARY BOGART

University of Illinois/Chicago Circle

*Forthcoming in *Ethics*.

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A PARADOX FOR COGNITIVE PSYCHOLOGY

(I-E: Cognitive Psychology)

The project of cognitive psychology rests on the principled autonomy of computational theorizing and its original explanatory power. To be scientifically legitimate and successful, computational explanation must carve up its domain at its natural and lawful joints. So the question for cognitive psychology is: What are the natural and lawful joints of cognition? What aspects of mind instantiate them? The most plausible answers turn out to be the most inimical to the project of cognitive psychology. The reason is this. The natural and lawful joints of cognition belong to the modular part of the mind. But this is precisely that part which can be conceptualized and explained by hardware and design theories whose descriptions and generalizations can be shown to be different from and more fundamental than those of cognitive psychology. The latter, then, faces the threat of either reduction or elimination. So we have a paradox: what makes cognitive psychology explanatorily legitimate and successful, as a science, also appears to make it ephemeral and dispensable since it can be either reduced to or eliminated by more fundamental sciences of the mind.

RADU J BOGDAN

Tulane University

MACINTYRE, NIETZSCHE, AND THE UNKNOWN ARISTOTLE

(II-F: Ethics)

In *After Virtue*, Alasdair MacIntyre[1] argues for a renewal of the "tradition of the virtues." In the wake of the failure of the Enlightenment project to provide a rational justification of objective morality, we are left with a choice between Nietzsche and Aristotle. But MacIntyre can himself accept only a truncated Aristotelianism: in his attempt to update the tradition of the virtues, he insists that such key Aristotelian doctrines as the unity of virtues, contemplation as telos, and, above all, Aristotle's "metaphysical biology" must be jettisoned. In this paper, I argue that this truncated Aristotelianism represents a repudiation of Aristotle just as great as that of the seventeenth century and that MacIntyre's own conception of moral life should be viewed as a contribution much more to the Hegelian tradition than to the Aristotelian tradition. Moreover, I suggest that, in pursuing MacIntyre's project of a renewal of the Aristotelian tradition, the thinker who might have the most to contribute is none other than Nietzsche; for Nietzsche's distinction between active and reactive will to power gives us a way of understanding and building upon precisely those crucial Aristotelian doctrines jettisoned by MacIntyre.

THOMAS BRIDGES

Montclair State College

[1] Notre Dame, Ind.: University Press, 1981.

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ART, ARTIFACTS, AND REGARDED INTENTIONALITY

(V-E: Aesthetics)

This essay examines in depth the condition that an art work must be intentionally produced. It is argued that for an object to qualify as an art work, it must be "regarded" as an artifact—where "regarding" is a distinctive propositional attitude not entailing belief—and that the work must further be regarded as having been created with only certain specific intentions, called "Recognition-implies-Fulfillment" intentions. The utility of treating the experience of an art work as an attribution of structured intentions to a "virtual" artist is examined for a wide range of aesthetic issues—interpretation, representation, expression, and the relationship of work and performance.

RANDALL R. DIPERT

State University of New York/Fredonia

HOW MUCH OF "REASON" DOES DESCARTES NEED TO VALIDATE?

(III-F: Modern Philosophy)

Descartes's apparent exemption of first principles from First Meditation doubt has been a source of much recent criticism. I argue that this exemption is justified on the grounds that First Meditation doubt is intended to apply only to those beliefs which a philosophically unsophisticated person can be aware of holding—a category which excludes first principles. I further argue that, although some aspects of "reason" are called into doubt by the deceiver argument, intuition and first principles are immune. Interpretations under which first principles are called into doubt take "reason" as a single faculty or activity, and "clear and distinct ideas" to be a single class, which must be either entirely trusted or entirely subjected to doubt. I argue that such views are mistaken.

LOIS FRANKEL

Ohio State University

HOW THE FINITE FOLLOWS FROM THE INFINITE IN SPINOZA

(III-F: Modern Philosophy)

Given the Spinozistic duality between the eternal world and the mutable world, a systematic ambiguity affects key terms such as 'necessity', 'follows from', and even 'infinite intellect', which thereby requires a bifurcation of key propositions in the *Ethics*. For example, Spinoza's statement of the Principle of Plenitude, in Proposition 16, of Part I, bifurcates as follows:

16(L) From the *logical-metaphysical* necessity of the divine essence infinitely many actualized *finite formal essences*, as well as *infinite*

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modes, logically-metaphysically follow in infinite ways (that is, all finite formal essences and infinite modes that can fall under the infinite *immediate* intellect).

16(C) From the *causal* necessity of the divine essence infinitely many *finite mutable things causally* follow in infinite ways (that is, all finite mutable things that can fall under the infinite *mediate* intellect).

Letter 64 to Schuller is invaluable in justifying this bifurcation.

JOEL FRIEDMAN

University of California/Davis

AUTONOMY AND THE SPLIT-LEVEL SELF

(II-G: Persons and Purposes)

'Split-level self' is the term I use for a view of the self which: (1) regards motivations and critical assessments of motivations as two different "levels" of phenomena characterizing the self; (2) assumes that motivations are determined by factors other than the self; and (3) argues that autonomy is achieved by the self in virtue of the endorsement of motivation at the level of critical assessment, the self, accordingly, identifying with the endorsed motivations as its "true" self. Against this account of autonomy, I argue that: (1) problems arise when critical assessment, as conceived on this account, is regarded both as a necessary condition for autonomy and as more truly of the self than unassessed motivation; (2) the notions of "true" self and of "identification" with parts of oneself are not coherent notions; and (3) although critical assessment may be a necessary condition for autonomy, the split-level account of this is misleading because it utilizes an unduly narrow conception of critical assessment, and neglects other equally necessary conditions of autonomy.

MARILYN A. FRIEDMAN

Bowling Green State University

DOMINATION AND SELFHOOD

(I-G: Alienation, Mind, and Technology)

Accounts of legitimation in advanced capitalism have stressed what people *believe*—e.g., about the state or the economy. My concern here is with what people feel about themselves and how a "dominated self" supports contemporary forms of social power in the United States. With the help of concepts derived from Hegel, Sartre, and Richard Sennet, I will describe certain features of this self, paying special attention to the manner in which a sense of personal unreality makes collective political power difficult to achieve even during periods of legitimation crisis.

ROGER S. GOTTLIEB

Worcester Polytechnic Institute

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FROM REALISM TO FOUNDATIONALISM IN C. I. LEWIS

(IV-F: American Philosophy)

Anyone who examines C. I. Lewis's doctrine of the given is likely to find difficulty in understanding and bringing into coherence his diverse remarks on the subject. This difficulty results primarily from the fact that Lewis used the expression 'the given' to refer to two quite different ideas, but failed to recognize the difference between them. Thus, there are two doctrines of "the given" in Lewis. According to the first, 'the given' refers to that element in perceptual experience which is independent of the conceptual activities of the mind. To defend the given in this sense is to defend realism. According to the second doctrine, 'the given' refers to that element in perceptual experience which makes possible a foundation of empirical knowledge. To defend the given in this sense is to defend foundationalism.

My purpose in this paper is to explain these two senses of the given, and to trace their development from Lewis's 1910 Harvard Ph.D. dissertation to his 1946 *An Analysis of Knowledge and Valuation*. It will be shown that, though the realist sense of 'the given' is found throughout, the foundationalist sense emerges clearly only in *Analysis*. Thus there was a transformation in Lewis's understanding of this key concept.

CHRISTOPHER GOWANS

Fordham University

DEMOCRATIC CAPITALISM

(II-E: Social Philosophy)

One way of ensuring that individual actions do not violate a group's moral norms is to develop within each individual a conscience. Conscience consists in the internalization or acceptance of a group's moral norms as correct and overriding one's self-interest when they conflict.

Corporations as well as individuals need a conscience to monitor and control their behavior. The correlative of a personal conscience in a corporation consists in the representation of group interests in the running and managing of the firm. This means that consumer and employee representation on the board of directors and in management is the most effective way to promote corporate moral behavior.

JOSEPH M. GRCIC

Loyola University of Chicago

THE IMPACT OF WORD-PROCESSING TECHNOLOGY ON HUMAN THOUGHT

(I-G: Alienation, Mind, and Technology)

Philosophical examination of the nature of "artificial intelligence" has tended to eclipse the converse question concerning the impact of computer

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technology on the human thought process. This paper is a discussion of the philosophical significance of word-processing technology. First, I review the current interest of philosophers in language transformations and in the correlative changes in the formulations of thought. Then, a dominant approach to such transformations—represented by Eric Havelock and Walter J. Ong—is examined and found wanting for its lack of critical capability. Finally, an alternate model of thinking is posed, one derived from the classical philosophy of mind. The latter is developed as a contrastive model upon which to build a critical and philosophically sound assessment of word-processing technology.

MICHAEL HEIM

Missouri Western State College

UNDERSTANDING BUTLER

(IV-D: Modern Philosophy)

I fret over an enthymematic argument in Sermon 11 which concludes that "all particular appetites and passions are toward *external things themselves*, distinct from the pleasure arising from them." The conclusion is, in Butlerian, a tautology; it is non-identical with what actually follows from Butler's stated premises; ever so gently nudged from tautologicality, it becomes suddenly too strong. I suggest some diagnoses and charitable interpretive moves, but cannot make the argument fully worthy of the insight which it does not quite express.

RICHARD G. HENSON

Rutgers University

METAPHORS OF TRUTH

(III-E: Language, Poetry, and Truth

Most philosophers would argue that metaphors cannot be both irreducibly metaphorical (i.e., not simply equivalent to a series of literal comparison statements) and true in a philosophically interesting sense (i.e., apart from the occasional metaphorical claim which by chance is also literally true); consequently, metaphors are excluded from the domain of knowledge, since being true is a necessary characteristic of genuine knowledge. Instead of revising our understanding of metaphor to make it more closely correspond to our idea of truth and thereby open the way to an appreciation of the epistemic status of metaphors, I argue that our idea of truth is itself often a metaphor. The two dominant traditional theories of truth, the correspondence and coherence theories, both rest upon fundamental metaphors apart from which they are untenable. Once the fundamentally metaphorical character of these theories is appreciated, the objection that metaphors cannot enter into the domain of knowledge because they cannot meet the standards of either the correspondence or coherence theories of

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truth loses its force. If metaphors cannot be true, then philosophy itself would be forced to abnegate its claim to truth.

LAWRENCE HINMAN

University of San Diego

ANTI-REALISM IN KRIPKE'S WITTGENSTEIN

(I-F: Realism and Relativism)

With the publication of his *Wittgenstein on Rules and Private Language: An Elementary Exposition* Saul A. Kripke[1] has redirected attention to themes in Wittgenstein's later philosophy. Kripke writes: "Wittgenstein has invented a new form of skepticism. Personally I am inclined to regard it as the most radical and original skeptical problem that philosophy has seen to date (60)."

The main objective of this presentation is to suggest ways of following Kripke in a reconsideration of Wittgenstein's profound form of skepticism and so-called "skeptical solution" for it; the paper is divided into three parts. First a series of arguments is developed to expose, for future examination, a variety of assumptions used in the generation of Wittgenstein's profound skeptical problem about the very possibility of intelligible speech and thought. The second part characterizes Wittgenstein's solution; it shows that intelligible language, and hence thought, is possible only if communities through their ways of life provide assertion conditions for claims. Assertion conditions are contrasted with truth conditions. It is argued that a consequence of Wittgenstein's solution is that whatever thinks is essentially a plurality. In the third part it is argued that Wittgenstein's provocative position that contradictions do not reveal a deep disorder in mathematics is a consequence of his skeptical solution.

CHARLES F. KIELKOPF

Ohio State University

FUNCTIONALISM AND MASOCHISM

(V-F: Philosophy of Mind)

I will pursue a new development of a well-known problem for the theory of mind known as functionalism—the view that mental states are to be individuated by their functional (causal) roles. This is the *qualia problem*: How can functionalist theory provide an adequate account of the qualitative (feeling) aspect of experience? This problem has been forcefully presented by a number of authors who have constructed elaborate examples in which, so our intuitions seem to tell us, functionalist theory is satisfied, but the intuitively requisite sensory qualia either are not present or are not present in the way common sense requires. Needless to say, these objections have provoked considerable response. What I want to introduce into

[1] Cambridge, Mass.: Harvard, 1982.

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this controversy concerns an abnormal phenomenon, but not a purely fanciful one, namely that of masochistic behavior. The question I want to raise is this: How is masochism to be explained by functionalist theory? In what follows I will first set forward the problem. I will then argue that it is importantly different from other standard qualia objections that have been presented. Thirdly, I will consider what I take to be some inadequate attempts to solve the problem. Finally, I will suggest a further general worry that the phenomenon of masochism suggests for the functionalist approach to the mind.

ERIC RUSSERT KRAEMER

University of Nebraska/Lincoln

BUILDING THE BODY POLITIC

(V-E: Aesthetics)

Architecture exerts a powerful political influence on society, helping determine the extent to which people are politically empowered or impoverished. It helps determine whether they constitute a public: collectively deciding matters of common concern in a self-conscious way. Architecture—broadly construed to include the built environment in general—exerts this influence by facilitating or frustrating social interaction and individual autonomy. An examination of contemporary building tendencies and alternatives to them supports this politico-architectural thesis. Thus, the linear traffic and building design of the highway "strip" isolates people, whereas medieval and condominium courtyards foster interaction. In a parallel way, the "picturability" of midtown Manhattan's street grid fosters autonomy by facilitating choice in movement, but the jumble of Jersey City's streets does not. Consideration of increasing political alienation, therefore, should include the way much of our built environment conspires against social interaction and individual autonomy.

JOSEPH KUPFER

Iowa State University

ALIENATION IN A MEDIATED WORLD

(I-G: Alienation, Mind, and Technology)

I introduce the concepts of mediation and psychic distance to give a descriptively accurate, nonnormative account of alienation phenomena. Mediation, the performance of actions on each others' behalf, is an endemic feature of populous and industrially advanced societies. What we frequently refer to as "alienation" is due to this structural trait rather than to any specific malformation of the social whole.

I show how manipulativeness, passivity, diminished responsibility, and the sense of impotence are natural consequences of large-scale mediation. They constitute the costs of the social life we lead. This theory is testable: I

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generate four theorems to indicate how it can lead to an empirical research program.

JOHN LACHS

Vanderbilt University

TOLERANCE, COOPERATION, AND RESPECT FOR WRONG VIEWS

(III-G: Ethics)

I distinguish tolerance from ideas with which it is sometimes confused, as well as from the related attitudes of cooperation and respect. I argue that, in spite of a seeming paradox, respect for a position you regard as wrong is compatible with the principle (universalizability) that many philosophers use to explicate the impartiality of morality. Thus I argue that one can say, "I think you are wrong, but you should do what you think is right" while still affirming a nontrivial version of universalizability. I conclude with a discussion of the justifiability and coherence of combinations of attitudes toward actions and positions with which one disagrees. My intention is to show that thinking through the appropriateness of these attitudes will help one live one's convictions while being civilized to those with whom one disagrees.

EDWARD A. LANGERAK

St. Olaf College

MENTAL REPRESENTATION FROM THE BOTTOM UP

(I-E: Cognitive Psychology)

Mental representation is a central concept in cognitive science. I argue that straightforward considerations of the nature of representation and intentional explanation constrain the possible successful methodologies in cognitive science. Those constraints recommend a "bottom-up" approach which includes the inner physiological states of organisms in their causal environmental contexts. I then outline a theory of representation which has some affinities to information theory and which is consistent with what is known of the neurophysiology of simple nervous systems. I close with thoughts on how simple representations might combine in higher cognition.

DAN LLOYD

University of California/Santa Barbara

FETAL STATUS AND WOMEN'S ROLES IN THE ABORTION DEBATE

(III-G: Ethics)

In this paper I examine arguments regarding the status of the fetus which depend on (1) its potentiality and (2) its status in relationship to women's

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roles. I point to strong reasons (some suggested by Steven L. Ross and some by myself) which a woman might have for wanting to have an abortion that would kill a fetus that would otherwise become her child—even though she could elect an abortion of a sort that would allow its preservation. I argue that even the strongest reasons given would not provide any good moral grounds for killing the fetus if the *actual personhood* of an early fetus were established—for this would entail the presence of the nurturing obligations women are thought to have toward their offspring. I conclude by giving reasons to believe that features of the nurturing situation in a strangely analogous case involving moral patients gives prima facie grounds for believing that, in cases of early fetuses, no such obligation exists.

RUTH LUCIER

Bennett College

STICH'S BELIEF SKIT

(III-D: Folk Psychology versus Cognitive Science)

Prominent theories in the intersection of cognitive psychology and the philosophy of mind feature the notion of the content of mental representations generally and belief particularly. These theories typically want an explication of the notion of content. Stephen Stich[1] has recently offered an analysis of the ascription of belief which is designed to satisfy this want. Though I think that there is much to learn from Stich's proposal, I argue that it is subject to two objections. On the one hand, it is inconsistent with a variety of first-person belief ascriptions, and, on the other hand, it circularly presupposes the very concept under analysis. If I am right, skeptical conclusions regarding the status of belief in cognitive science wrung from Stich's account of belief attributions need not be endorsed.

J. CHRISTOPHER MALONEY

Oakland University

THE REPRESENTATIONAL-COMPUTATIONAL THEORY OF MIND

(I-E: Cognitive Psychology)

How should we understand the mind, when studying the mental causes of behavior? Should we, in particular, say the mind's structure is both representational (mental states consist of relations between organisms and representations) and computational (mental states can be specified without reference to the semantic properties of the representations over which they are defined)? I argue that we should not. The mind may be representational, but it most certainly is not computational.

PETER J. MARKIE

University of Missouri/Columbia

[1] *From Folk Psychology to Cognitive Science* (Cambridge, Mass.: MIT Press, 1983).

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GENDER AND ESSENCE IN ARISTOTLE

(V-D: Ancient Philosophy)

That Aristotle should have been inclined to think of women as defective men is, perhaps, overdetermined. But the psychological and sociological factors that contributed to his male chauvinism should not be allowed to obscure the role his metaphysics played in the way he conceived gender difference. Basic metaphysical constraints left Aristotle with no entirely satisfactory way of understanding gender identity.

GARETH B. MATTHEWS

University of Massachusetts/Amherst

UNDERSTANDING FREQUENCY-DEPENDENT CAUSATION

(V-G: Convergence, Frequency, and Probability)

Investigating causal hypotheses in science may involve effects that vary according to the relative frequency of the causal factor of interest. Such causal effects are said to be *frequency-dependent*. Cases of frequency-dependent causation have appeared in recent discussions of the causal model proposed by Ronald Giere.[1] Eliot Sober claims to show how certain cases of frequency-dependence (in evolutionary theory) "constitute counterinstances of Giere's proposal." In Giere's recent response, he acknowledges that his original analysis was not intended to cover frequency-dependent cases, but suggests how such cases may be accommodated by extending his original analysis. John Collier maintains that Sober-type examples of frequency-dependence merely show Giere's original analysis to be ambiguous, and suggests qualifications to avoid this. In the present paper I shall (1) explain the problem that allegedly arises in applying Giere's original analysis to frequency-dependent cases; (2) argue that the solutions sketched by Giere and Collier, though keeping out some of the problems Sober raises, open the door to yet others; and (3) argue that all these problems are avoided by Giere's initial analysis, provided that it is carefully and consistently interpreted.

DEBORAH G. MAYO

Virginia Polytechnic Institute & State University

PUTNAM'S VAT WORLD AND THE SKEPTICAL METHOD

(V-F: Philosophy of Mind)

Hilary Putnam's well-known argument that it is a necessary truth that we are not brains in a vat[2] illustrates the skeptical method of dealing with a

[1] "Causal Systems and Statistical Hypotheses," in L. J. Cohen and M. B. Hesse, eds., *Applications of Inductive Logic* (New York: Oxford, 1980), pp. 251–270.
[2] *Reason, Truth and History* (New York: Cambridge, 1981), p. 15.

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problem about representation. Because of this method, Putnam is faced with the dilemma that his argument is either useless or superfluous. I argue that it is superfluous and that the skeptical method of handling the problem turns on treating a contingent truth—such as that I am not a brain in a vat—and the logical possibility of its negation as if they were inconsistent. The skeptical method requires us to secure a contingent truth in the face of an illusory inconsistency. Putnam, however, considers the proposition that I am not a brain in a vat a necessary truth. If my diagnosis is right, then his argument should not be sound. In fact it is invalid.

JEREMIAH E. MCCARTHY

University of Missouri/Rolla

HABERMAS AND THE LANGUAGE OF POETRY

(III-E: Language, Poetry, and Truth)

Jürgen Habermas has suggested that literary discourse, like other forms of speech, claims to be true, sincere, socially appropriate, and intelligible. After criticizing this, I suggest that the concept of "situation-building speech" developed by Wolfgang Iser and Hans-Robert Jauss can be accommodated to Habermas's framework. The result is a type of utterance which differs from Austinian models in that it is not governed by a single set of conventions; rather, it is up to the hearer of the utterance to decide what conventions are to be applied to it. Such speech, though not confined to the literary sphere, shares certain features with it: it is relatively poor at conveying information, and relatively good for constructing fictional and real situations. It is a "proto-aesthetic" aspect of ordinary language.

JOHN MCCUMBER

Northwestern University

PLATONISM AND THE ITERATIVE CONCEPTION OF SET

(II-D: Philosophy of Mathematics)

In this paper I argue that certain essential features of the iterative conception make it unacceptable for use by a platonist. That is, if we rely on the iterative conception of set to justify and motivate our view of the structure of sets, that structure cannot be interpreted in a platonistic or realistic way. I distinguish two ways of reading the iterative conception, one Cantorian and the other minimalist. The Cantorian conception is too profligate with its justification of higher cardinals for the universe to be closed under the iterations permitted. The minimalist reading does not have this problem, but does have one very similar. On it, one needs to justify from other sources the existence of larger sets. The nature of that justification is such that there is no end of sets that can be justified.

CLIFTON MCINTOSH

University of Utah

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ON JUSTIFYING AND DIFFERENTIATING RIGHTS

(IV-E: Political and Legal Philosophy)

Ronald Dworkin maintains that particular rights, like the right to free speech and the right to own personal property, can be derived from a foundational right, the right to equal concern and respect.* This paper questions the tenability of this program for rights-based rights. A right is an individuated moral or political guarantee which confers a specified benefit on each right-holder and which resists conduct that would derogate it. For there to be rights-based rights, both the foundational right and the rights it implies must satisfy this definition. It is doubtful, however, that the right to equal concern and respect should count as a right, since the benefits it confers are at best highly controversial and may not be assignable to individuals. But even if we grant that the content of the right to equal concern and respect can be satisfactorily specified, the status of the derived rights remains problematic. The trouble is that the relation between the right to equal concern and respect and the rights it implies parallels the relation between the principle of utility and the rights it may imply. Both of these foundational principles can extinguish derived rights. Consequently, rights dependent on either of these principles are not "trumps," and their standing as rights is suspect. I conclude that Dworkin's method of defending rights is inappropriate for the most important of our rights though it may well serve for less critical ones.

DIANA T. MEYERS

Cornell University

PHENOMENOLOGY AND RELATIVISM

(I-F: Realism and Relativism)

Phenomenology originally claimed to have decisively refuted relativism. But, as the early essentialism yielded to the later interest in constitution of meanings, relativism appeared from within phenomenology. How can a phenomenological philosophy deal with relativism? Instead of refuting it, I suggest, one must be able to go through it as far as one can, and then go beyond it. For this purpose, I make use of three strategies: (a) the principle of correlation between noesis and noema; (b) the idea of overlapping noemata, and (c) the idea of translatability in a dynamic sense. These help us to accept relativism provisionally, but also to delineate a path that could lead us out of it. Such a path would be guided by the regulative Idea of *one* world.

J. N. MOHANTY

University of Oklahoma

*_Taking Rights Seriously_ (Cambridge, Mass.: Harvard, 1977), p. 180.

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PLATO ON TIME AND ETERNITY

(V-D: Ancient Philosophy)

The paper gives an interpretation of Plato's remarks on time in the *Timaeus* (38c–39c) which, in turn, recommends a certain interpretation of Plato's claim that Forms are eternal. I argue that, when he says that god makes time, what Plato really means is that god makes a clock or system of clocks: standards by which we measure that which is measurable about motion and rest. I then argue that the sense in which this "time" is eternal is that, qua standard, it itself is outside of time in that it fails to fall within the category of things of which it is intelligible to predicate dates and durations. It is this sense of eternity, not eternity as permanence, which then is applied to Forms qua standards. Along the way, I suggest that Plato's conceptions of time and eternity are more coherent than they usually have appeared to critics.

RICHARD MOHR

University of Illinois/Urbana-Champaign

REALISM, TRUTH, AND REFERENCE

(V-G: Convergence, Frequency, and Probability)

Recent philosophical accounts of scientific realism cast it as an essentially epistemological thesis; indeed, the most prominent recent accounts tie the thesis of scientific realism to the doctrine of *convergence*. Convergence is an epistemological and semantic thesis to the effect that our mature scientific theories are approximately true, that their central terms refer, and that the history of science reveals a picture of ever-advancing theories, with each subsequent theory ever more closely approximating the truth. Convergence has been challenged by those who have offered case studies of past established theories that did not converge. This challenge to convergence is supposed to raise serious doubts about the tenability of scientific realism. Although these historical cases may be telling for those who regard convergence as an essential part of scientific realism, we argue that convergence and realism are logically and actually independent. We then raise the more general question whether an epistemic component is a necessary or desirable part of realism, and conclude that it is neither. On the contrary, the more fundamental and important versions of scientific realism do not construe it as an epistemic or semantic thesis. It is, rather, an ontological or metaphysical claim about the entities posited in scientific theories and about the truth conditions for theoretical claims.

W. E. MORRIS

ROBERT C. RICHARDSON

University of Cincinnati

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UNIQUENESS AND PLATO'S THIRD BED ARGUMENT

(V-D: Ancient Philosophy)

In this paper I offer an interpretation of the argument at the beginning of *Republic* X (597c1-d3). The argument—sometimes called the Third Bed Argument (TBA)—shows that the Form of bed is unique. In this argument Plato enunciates a version of the One-over-Many principle (OM), which justifies positing Forms, and then applies the principle. The argument assumes two other principles as well, which are supplied in the interpretation. After adding these assumptions, one can see that neither explicit premises nor assumptions imply an infinite regress. The fact that the TBA is not liable to an infinite regress points up an important contrast between the TBA and the first Third Man Argument (TMA) of the *Parmenides* (132a1-b2); this contrast allows us to see that the TMA is not the revelation of a fatal flaw in the theory of Forms of the middle dialogues, as it is sometimes claimed.

RICHARD D. PARRY

Agnes Scott College

THE MEANING OF THE PURPOSES OF LIFE

(II-G: Persons and Purposes)

I suggest four conditions that must obtain before the presence of purpose(s) in life will assure meaning to that life. Briefly, the person must be aware of the purposes, must, indeed, have appropriated them as his/her own, and, on the basis of a suitable "fit" between him/herself and those purposes, must in fact be performing at least some actions that are related to the accomplishment of these purposes. There are two major objections to any suggestion that such a set of conditions is also sufficient to connect purposes with meaning, and the rest of the paper is devoted to attempts to answer them: first, that only special purposes, perhaps those sanctioned by some creator, would be sufficient to provide meaning to life; second, that this set of conditions does not leave room for meaning in *life* as such.

GALEN K. PLETCHER

Southern Illinois University/Edwardsville

RECURSIVE BEHAVIORISM

(V-F: Philosophy of Mind)

This paper makes evident the coherence of a view which avoids a traditional objection to behaviorism, namely that its attempts to attribute be-

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liefs and desires must be circular. Although a direct argument for the truth of the view is not feasible, support is offered for the truth of the base of the recursion. Further, the advantages of the view make it a candidate for the best explanation of our attributions of believing and desiring.

WILLIAM S. ROBINSON

Iowa State University

IS SCIENCE WITHOUT NUMBERS POSSIBLE?

(II-D: Philosophy of Mathematics)

In his book, *Science without Numbers*, Hartry Field advances the thesis that science without numbers is in principle possible, i.e. that numerical scientific laws and other numerical statements, although practically advantageous in computation and derivation, are theoretically replaceable by non-numerical counterparts which contain neither numerals nor other references to numbers. The paper shows that Field's thesis is not generally true by presenting a number of counterexamples, the most important of which are low-level numerical laws such as the Pythagorean theorem and Galileo's law of falling bodies. Apparently these laws are not, in the requisite sense, completely reformulatable in non-numerical terms. Hence, we may retain the view that science, as we know and practice it, is not even in principle possible without numbers. There may be things we do with numbers that we can do without them, but there are some predictions and calculations we make with numbers that we cannot make without them.

C. WADE SAVAGE

University of Minnesota

REALISM VERSUS ANTI-REALISM: A STATEMENT OF THE ISSUE

(I-F: Realism and Relativism)

The issue between realism and anti-realism is associated in contemporary philosophy with the work of Michael Dummett. Dummett approaches the issue through the problem of *truth*; his work appeals—in a manner disturbing to many philosophers—to the later views of Wittgenstein and to the verificationist principle; and it is complex and enigmatic. Consequently, the issue of realism and anti-realism has become shrouded in mystery—a mystery which I propose to dispel. A crisp statement of the issue, requiring no resort to metaphor, is possible. I shall state the issue in a way that makes no mention of the problem of truth or meaning, the views of the later Wittgenstein, bivalence, or verification. My proposal will be measured by the degree of clarity it brings to an area that has hitherto been lamentably obscure. I naturally risk stating a *different* issue, which might not interest Dummett or anyone else. So my plan also carries a recommen-

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dation: at bottom, I contend, *this* is the issue people are concerned with; this is how the issue ought to be understood.

DAVID SEDLOCK

Syracuse University

EXPECTED UTILITY AND RISK

(V-G: Convergency, Frequency, and Probability)

The rule to maximize expected utility is designed for decisions where options involve risk. Attitudes toward risk are important factors in such decisions, and the rule ought to take account of them. Allais's and Ellsberg's paradoxes, however, suggest that the rule ignores attitudes toward risk. This suggestion is supported by recent psychological studies of decisions. They present a great variety of cases where apparently rational people violate the rule because of aversion or attraction to risk. Here I argue that the standard version of the rule to maximize expected utility does indeed neglect attitudes toward risk. I construct a new, general argument that proves this in a rigorous way.

PAUL WEIRICH

University of Rochester

UNPERCEIVED OBJECTS AND BERKELEY'S DENIAL OF BLIND AGENCY

(IV-D: Modern Philosophy)

What happens to Berkeley's table when there is no one in the study? One answer is that the table continues to exist because God perceives everything we don't. A second answer is that the table remains because Berkeley *would* perceive it if he entered. The second answer emphasizes Berkeley's phenomenalism and represents, I think, his considered view of real existence. In this paper I argue that the second answer actually entails the first. My argument turns on the *denial of blind agency*, a commonplace of seventeenth-century philosophy of mind (endorsed by Descartes, Malebranche, and Locke), which holds that a spirit cannot will what it does not perceive. The denial not only permits us to correct and reconcile the competing interpretations of Berkeley on unperceived objects, but suggests that Berkeley does not (as Jonathan Bennett contends) conflate causation and inherence in one of his proofs of God's existence. The denial also provides a suitably modest interpretation of Berkeley's views on archetypes.

KENNETH P. WINKLER

Wellesley College

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WHITHER FOLK PSYCHOLOGY?

(III-D: Folk Psychology versus Cognitive Science)

Several authors, notably Paul Churchland and Stephen Stich, have recently argued that the common-sense concept of belief is incoherent from the point of view of cognitive theory, and hence that it will likely be eliminated from the science of psychology, just as concepts such as "pneumata" and "aether" were eventually eliminated from other sciences. I shall argue that the reasons they advance in support of this claim are unpersuasive and that their view ignores possibilities other than the eliminativist option. Specifically, I will suggest that eliminativism can be avoided by adopting a position in which propositional attitudes are viewed as supervening on underlying computational cognitive structures. Although such an alternative does not foreclose the possibility of eliminativism being true, it offers the possibility of reconciling significant features of folk psychology with the requirements of serious cognitive science.

MORTON E. WINSTON

Trenton State College

REMOTE OBLIGATION

(II-F: Ethics)

A distinction is drawn between two types of obligation, immediate and remote. The distinction is used to shed new light on the issues of whether "ought" implies "can" and of whether there can be any genuine moral dilemmas. Attention is given to a type of moral dilemma that seems mostly to have escaped notice, a type which is given the name "moral bind."

MICHAEL J. ZIMMERMAN

Brown University

The editors note with regret that it has proved impossible to include the Appendix to the APA Program in this issue of the *Journal of Philosophy* (see LXXXI, 10, October 1984, page 624). This issue would not accommodate twelve more pages. Our readers are referred to the November issue of *Proceedings and Addresses of the American Philosophical Association* for information about group meetings and group participants.

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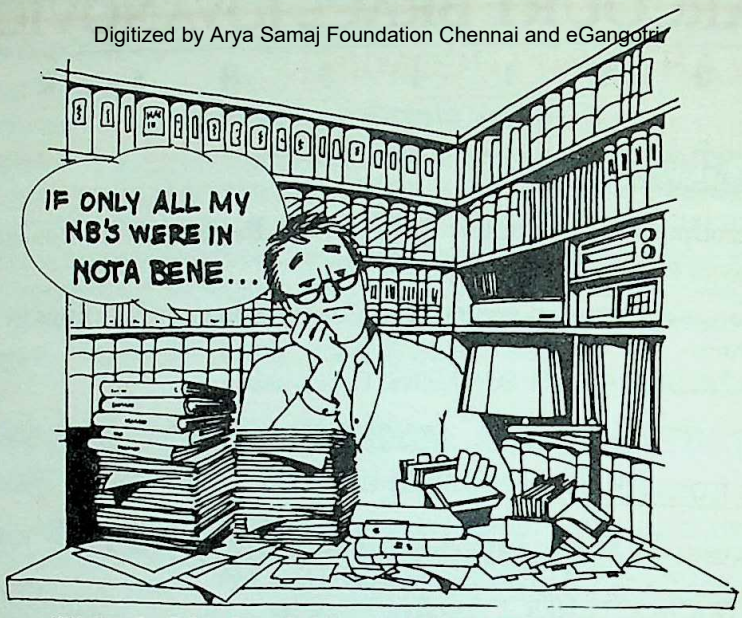

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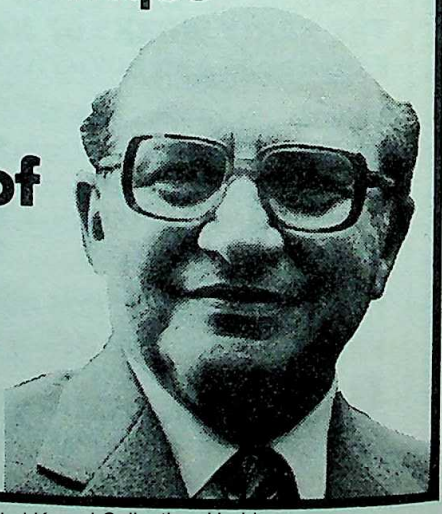

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COMPUTATIONAL COMPLEXITY AND THE UNIVERSAL ACCEPTANCE OF LOGIC*

WHEN W. V. Quine says, "better translation imposes our logic" upon the beliefs of any agent we try to interpret, and, furthermore, "the logical truths, or the simple ones, will go without saying; everyone will unhesitatingly assent to them if asked,"[1] it is natural to wonder about the universal acceptance of logic. Let us consider the following version of the thesis: Any rational agent must accept a logic, that is, at least a sound and complete first-order deductive system. I shall argue that the thesis is false under some natural and philosophically important interpretations. The discussion identifies relationships between computational-complexity theory, recent psychological studies of the formal incorrectness of everyday reasoning, and more realistic theories of rationality.

Prima facie, the pattern of complexity-theoretic results in recent years constitutes a kind of practical analogue of the classical absolute unsolvability theorems of the 1930s. The project that emerges is to find the philosophical implications of these results, just as we have been trying to interpret the classical unsolvability results. In particular, if complexity theory in some sense "cuts the computational universe at its joints"—providing a principled basis for a

* This paper was prepared with support from the American Philosophical Society and the National Endowment for the Humanities. Lenore Blum, Charles Chihara, William Craig, Daniel Dennett, Richard Karp, and Barry Stroud generously helped at various stages of the paper. Some of this material appeared in my Ph.D. dissertation, "Belief and Logical Abilities" (Berkeley: University of California, 1977); other material was presented at the University of California, Berkeley, Logic and Methodology Colloquium, February 1982. The paper was read to the Berkeley Philosophy Colloquium, June 1982.

[1] See, respectively, *Word and Object* (Cambridge, Mass: MIT Press, 1960), p. 58; and *Philosophy of Logic* (Englewood Cliffs, N.J.: Prentice-Hall, 1970), p. 102. (Some of Quine's discussion of logic and translation can be construed as entailing only the universal *nonrejectability* of logic. However, the argument below implies that correct translation might attribute rejection of any particular logical truths.)

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hierarchy of qualitative distinctions between practically feasible and unfeasible tasks—then we need to examine the idea that, in at least some interesting cases, rationality models ought not to entail procedures that are computationally intractable. Complexity theory raises the possibility that formally correct deductive procedures may sometimes be so slow as to yield computational paralysis; hence the "quick but dirty" heuristics uncovered by the psychological research may be not irrational sloppiness, but instead the ultimate speed-reliability tradeoff to evade intractability. With a theory on nonidealized rationality, complexity theory thereby "justifies the ways of Man" to this extent.

To begin, what is, or would be, a universally accepted logic? At a minimum, the thesis would be that all rational agents accept *some* sound and complete set of laws or axioms and inference rules for first-order logic, as opposed to a claim of the universal acceptance of a particular set of "fundamental" logical laws and rules, or even more strongly, a claim of universal acceptance of all logical truths. The weakest thesis, therefore, could be false either in that: (a) an agent might not accept a complete deductive system; he might have a "cognitive blind spot"; or (b) the agent might accept only an unsound or even inconsistent system; e.g., he might use some rule that did not guarantee preservation of truth in inference, perhaps a "quick but dirty" heuristic; or, more strongly, (c) every law or rule the agent used might be unsound or also inconsistent. I shall deal almost entirely with classical logic. This is not to prejudge the issue of the adequacy of nonstandard logics; the case of classical logic is basic, and the argument should be generalizable to other logics. Whatever one's choice of logic, the prior, and usually unacknowledged, question is whether a sound and complete logic by any standard is in fact the best choice.

I. IDEAL RATIONALITY

Let us examine the concept of a rational agent which is involved in the thesis of the universal acceptance of logic. Some rationality constraint on an agent's cognitive system of beliefs and desires is the most fundamental law of psychology, more basic than any low-level empirical generalization. It is generally recognized in this philosophy of psychology that, for instance, although consistency may be the hobgoblin of small minds, consistency is a condition for having any mind at all: no rationality, no agent. The conventional strategy in the cognitive sciences has been first to adopt a rather extreme idealization of the rationality required of an agent and, then, perhaps, if they are noticed, to explain away departures of real human behavior from the ideal model.

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The models of the agent prevalent in decision, game, and economic theory and in philosophy require that the agent be a maximizer of expected utility, that is, that the agent A satisfy an ideal general rationality condition, a version of which is: If A has a particular belief-desire set, A would undertake all and only actions that are apparently appropriate.[2] Here an action is "apparently appropriate" if, according to A's beliefs, it would tend to satisfy A's desires. An agent who is able to choose his actions so well has to have a great deal of logical insight. In particular, he must satisfy an *ideal inference condition*:

> A would make all deductively sound inferences from his belief set which are apparently appropriate and would not mistakenly make any unsound ones.

Otherwise, A might miss some apparently appropriate actions, for instance.

Now, must an ideally rational agent in this sense accept a logic? For the agent to be able to perform all sound inferences that might turn out to be apparently appropriate and not to make unsound ones, he must meet Cartesian standards of perfection: he must in effect be both infallible and able to have an opinion on anything with respect to logic. Such an agent cannot accept an unsound or incomplete deductive system. If he accepts an unsound system for making some of his inferences, he will not be guaranteed to make only sound inferences, appropriate or otherwise; and if he accepts an incomplete system, he will not be able to make some sound inferences that might turn out to be appropriate. In either case, he will not satisfy the ideal inference condition. Therefore, an ideal agent must accept a sound and complete logic if he is to perform required reasoning by means of a formal deductive system.

II. UNDECIDABILITY

If a rationality requirement is the most basic psychological law, the next most basic fact of our psychology is that we are finite objects. The ideal rationality conditions abstract from this fundamental fact of human existence: we are in the finitary predicament of having fixed limits on our cognitive resources, in particular, on our memory capacity and computing time. The standard model in effect assumes, for such purposes as simplification of theory, that a human being has God's brain; for such an ideal agent much of the deductive sciences would be trivial.

[2] See my "Minimal Rationality," *Mind*, XC, 358 (April 1981): 161–183. (If the requirement that A attempt *only* apparently appropriate actions is dropped, a random guesser will satisfy the ideal conditions, given enough time; see the minimal conditions below.)

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If we suppose the agent is finite, there is still another cost for the idea of an ideal agent using only formal deductive procedures. For ideal rationality requires more than just use of a sound and complete logic. The agent has to be able to use that logic very well, so well that any given first-order sentence can be formally proved or disproved in a finite number of steps; otherwise, the ideal agent would not be *guaranteed* always to succeed in making all needed sound inferences (e.g., any arbitrary inference he thought his survival depended upon), and also never to make unsound ones. No heuristic procedure for using the deductive system, however good, would suffice unless it was perfect, with no possibility of failure, and hence in fact algorithmic. (Nor could any recursive enumeration procedure suffice by itself, since the agent would wait forever without finding out that some inferences were unsound.) This finitely represented perfect formal ability, of course, would constitute a decision procedure for first-order logic, which Church's theorem demonstrates to be impossible.

Hence, the ideal rationality conditions are very ideal indeed, in that they entail either the most basic practical impossibility—the use of infinite resources—or else a logical contradiction, like a square circle. Of course, the ideal rationality model remains an indispensable simplification of computational reality for many situations—for example, as one norm or "regulative ideal" for evaluating quick and dirty procedures. But some care is required; using the idealization could be a bit as if Hilbert had retained the presupposition of formalism that all number-theoretic truths are formally provable "as a convenient approximation" in the face of Gödel's incompleteness theorem.

But perhaps the agent might accomplish deductive tasks by some entirely nonformal means, for instance, by immediate synthetic a priori intuition of the deductive relations among the propositions he believed. The agent might do this by direct, quasi-perceptual, Gödelian insight into an independent realm of Platonic entities, or by means of his transcendental ego, situated outside of space, time, causality, and so on, as Kant and intuitionists such as Brouwer have identified it. Conformance to the ideal-inference condition through such faculties of intuition may seem little better than doing so by means of an oracle or miraculously perfect luck in guessing; for example, physicalists and those committed to information-processing models of cognition will not be satisfied with even the form of explanations like these. These procedures remain in need of at least the outlines of a scheme of explanation: The al-

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ternative procedure has to be such as to guarantee inferential success nonalgorithmically. With intuitive access to a Platonic realm, it is no longer clear that an agent *is* restricted to finite cognitive resources, e.g., of time and space.

III. COMPUTATIONAL COMPLEXITY

In fact, the deductive ability of the ideal agent is even further removed from computational reality. Even where there is no absolute undecidability, a kind of practical undecidability seems to extend further down, to the most basic parts of logic, to the very core of the notion of computation. In some respects, it is as if Church's theorem applied even to the propositional calculus. Of course, a decision procedure exists for tautological soundness—for example, by use of truth tables. But although a tautology decision procedure is in principle possible, it now appears to be inherently "computationally intractable" and, in some sense, to be extremely unfeasible as a practical matter, e.g., to require computations for relatively simple cases that would exceed the capacities of an ideal computer having the resources of the entire known universe. What is the philosophical significance of such intractability?

The above tautology result is in the field of computational complexity, an area that has grown rapidly during the last decade and is yielding practical unsolvability results which may be as interesting in some ways for philosophy as the classical absolute unsolvability results of the 1930s.[3] (Perhaps philosophy has overlooked the field so far because of a tendency to conclude that if a problem is decidable in principle, then it must be trivial, at least for philosophy conceived of as a "pure" nonempirical discipline.[4]) In complexity theory, feasibility of an algorithm is evaluated in terms of whether its execution time grows as a polynomial function of the size of input instances of the problem. If it does (as does any familiar procedure for arithmetical addition, for example), the algo-

[3] Two key papers are S. Cook, "The Complexity of Theorem-proving Procedures," in *Proceedings of the 3rd Annual ACM Symposium on Theory of Computing* (1971): 151–158; and R. Karp, "Reducibility among Combinatorial Problems," in R. Miller and J. Thatcher, eds., *Complexity of Computer Computations* (New York: Plenum Press, 1972): 85–103. A recent review of part of the field is M. Garey and D. Johnson, *Computers and Intractability* (San Francisco: W.H. Freeman Press, 1979). Two easily accessible articles are H. Lewis and C. Papadimitriou, "The Efficiency of Algorithms," *Scientific American*, CCXXXVIII (1978): 96–109; and L. Stockmeyer and A. Chandra, "Intrinsically Difficult Problems," *Scientific American*, CCXL (1979): 140–159.

[4] See S. Kleene's discussion of the concept of a decision procedure in sec. 40 of *Mathematical Logic* (New York: Wiley, 1967) for examples of the relegation of such issues to the applied sciences.

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rithm is generally treated as computationally feasible. If it does not and instead increases faster, usually as a exponential function (as does exhaustive search of the game tree in chess, for example), the algorithm is generally regarded as intractable.

Such intractability turns out to a large extent to be independent of how the problem is represented and of the computer model (e.g., random-access or deterministic Turing machine) involved. Just as Turing computability is a formal explication of our intuitive notion of computability, polynomial-time computability can be viewed as one formal specification of a pre-theoretic notion of practical computability. As a first approximation, we can say that complexity theory thereby identifies some of the "natural kinds" of computational difficulty. We shall turn to the question of the "real-world relevance" of complexity theory later; at least important exceptions must be acknowledged to any rule of thumb that equates real-world feasibility with polynomial-time computability.

The ideal agent's procedure for determining whether or not a sentence is a tautological consequence of a set of premises yields a test of whether or not a sentence is truth-functionally consistent. In complexity theory, the latter question is known as the "satisfiability problem." Briefly, the relevant finding here is that the satisfiability problem is a member of the very large and important class of "nondeterministic polynomial time" (NP) problems, which are known to be solvable in polynomial time on a nondeterministic Turing machine, which is allowed to make "guesses" and in effect has an unbounded capacity for some parallel computations. A problem solvable by a nondeterministic Turing machine in polynomial time is solvable by a deterministic machine in exponential time. NP includes P, the class of problems solvable on a standard deterministic Turing machine in just polynomial time.

Most importantly, the satisfiability problem is "NP-complete": *any* NP problem can be efficiently reduced to the satisfiability problem. Each one of the wide variety of known NP-complete problems, numbering in the hundreds, is similarly convertible into any other. In this way, the satisfiability problem is a "universal" NP problem. NP-complete problems have not been proved inherently to require deterministic exponential time; this is the major unanswered question, a "Goldbach's conjecture", of the field, equivalent to the question whether NP $\neq$ P in that some NP problems are not in P. However, NP-complete problems are generally regarded as computationally intractable in this way, since only exponential-time deterministic algorithms for any of them are known, and because if they were not, so many important problems that have long

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resisted practical solution (such as the "traveling-salesman problem"[5]) would then all turn out to be tractable.

Thus, the ideal agent's perfect capacity even just to make all tautological inferences is the case *par excellence* of a problem-solving capacity that is strongly conjectured to require computationally intractable algorithms. Of course, a "quick but dirty" heuristic procedure for tautological inference will not necessarily yield such apparent exponential explosion of computation—presumably, that is how actual fallible human beings manage, as we shall see. But again, nonalgorithmic procedures would not suffice for the Cartesian perfection of the ideal agent, since *ex hypothesi* they cannot be guaranteed to work in all cases. A surprisingly small and basic fragment of the ideal agent's deducing ability seems by itself to require, for just a finite set of simple cases, resources greater than those available to an ideal computer constructed from the entire universe. There is another layer of impossibility between the idealization and reality, not merely minor exceptions.

IV. MINIMAL RATIONALITY

We can therefore say that, although use of an ideal rationality model is an understandable motivation for arriving at the thesis of universal acceptance of logic, some other argument still is needed for that thesis. Although ideal and more realistic models ought to coexist, for some purposes the idealization strategy seems an overreaction to the "no rationality, no agent" point. The alternative approach is to begin with a somewhat less idealized, more realistic model, of *minimal* rationality, where the agent's ability to choose actions falls between randomness and perfection. A minimal general rationality condition would be: If A has a particular belief-desire set, A would undertake some, but not necessarily all, actions that are apparently appropriate. For an agent to satisfy this condition, he must have some, but not ideal, logical ability—that is, he must satisfy a *minimal inference condition*:

> A would make some, but not necessarily all, sound inferences from his belief set which are apparently appropriate.

(A must also *not* attempt enough of the actions that are apparently inappropriate, and inferences that are unsound or apparently inappropriate.)

A useful feature of this less idealized model of rationality is that,

[5]The traveling-salesman problem, an NP-complete network design problem of considerable practical interest in operations research, is: given a set of cities on a map and all intercity distances, construct the optimum tour, i.e., the shortest round-trip route connecting all the cities by intercity links.

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as we shall see, it provides a philosophical framework for relating two areas of significant research during the last decade. One is the field of computational complexity just sketched. The other encompasses the many recent psychological experiments that suggest surprisingly ubiquitous use of prima facie sub-optimal "heuristic strategies", rather than formally correct procedures, in everyday intuitive reasoning.[6] Although each of these areas has arisen independently of the others, there seems to be a fundamental connection: (a) Complexity theory provides a principled basis for considering that human beings (indeed, any computational entities) may not be able to perform some very simple reasoning tasks in ways that are guaranteed to be correct. (b) The psychology of "irrationality" suggests how we can do these tasks, by showing something of how we in fact do them—by means of the "quick but dirty" heuristics. (c) The ideal rationality models are at best silent on the normative status of use of these heuristics; the minimal rationality model, to begin with, permits us to acknowledge the basic platitude that human beings are in the finitary predicament, and so *ought* to use some such heuristics—according to this conception, formally incorrect heuristics need not in fact be irrational at all. They are not just inadvisable or unintelligible sloppiness, because they are a means of avoiding computational paralysis while still doing better than guessing.

The increasing interest in computational complexity and also in psychological heuristics makes it important to establish the status of claims of human (or even inherent computer) alogicality or illogicality. In particular, are the claims somehow a priori incoherent and so not a matter open for empirical study, as the rationality idealizations—and even the usual "charity principles"—imply? We thereby return to the issue of the universal acceptance of logic. Given the limitations of the concept of the ideal agent, the main question has now become: If a supposed cognitive system qualifies as minimally rational, is there any sense in which it must include a logic?

To determine in what sense, if any, satisfaction of the minimal rationality conditions implies acceptance of a deductive system, we must ask: What is accepting (or believing) a logical law or rule? Briefly, let us distinguish between strong and weak acceptance of

[6] For a review of some of their own basic work by two major contributors to the field, see A. Tversky and D. Kahneman, "Judgment under Uncertainty: Heuristics and Biases," *Science*, CLXXXV (1974): 1124-1131. A recent overview is R. Nisbett and L. Ross, *Human Inference: Strategies and Shortcomings of Social Judgment* (Englewood Cliffs N.J.: Prentice-Hall, 1980).

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logic. Assent to a logical law, mere lip service, is not enough to constitute strong belief in the law. Assent is neither sufficient nor necessary, although it is one type of evidence for such acceptance. In addition, acting appropriately for, or reasoning in accordance with, a logical law is not enough to constitute such belief. For instance, a sound argument is "in accordance with" *every* valid sentence, in the sense that the argument's conclusion also follows from the premises conjoined with any of these validities; there is then no distinction with regard to accepting logic between idiot and super-savant.

As Donald Davidson has emphasized, a belief must be part of an agent's reason for a decision.[7] Causal efficacy, the "right" role in the decision-making process, also is required here; the minimal agent must actually use the law as a premise in some (not necessarily all) of the practical reasoning, conscious or unconscious, by which he would select apparently appropriate actions (There can be important "generate and test" interplay between heuristic reasoning in the context of discovery, and logic as *post hoc* tribunal in the context of proof.) The key notion in turn, therefore, is that of "using a logical law or rule" (the related notion of "following a rule" has received much attention[8]).

In contrast, to accept or believe a logical law weakly is merely to be usefully (or instrumentalistically) described as using the law; it may be clear that the agent is not in fact using the law at all. This appears to be the sense in which Daniel Dennett says of adaptively behaving creatures from another planet, "in virtue of their rationality they can be supposed to share our belief in logical truths," and further, of "mice and other animals, in virtue of their being intentional systems," "whether or not the animal is said to *believe* the *truths* of logic, it must be supposed to *follow* the *rules* of logic."[9]

Thus, a person might strongly accept a logic—a small set of simple axioms and inference rules from which all logical truths could "in principle" be derived. But such strong acceptance of a complete deductive system for first-order logic is not the same as strong acceptance of "the theory of first-order logic": actual appropriate use of each of the infinitely many assertions derivable by means of those axioms and rules. However, although the latter is not possible for a realistic or minimal agent, the agent can resolve to accept, or be

[7] See, for example, "Psychology as Philosophy," in *Essays on Actions and Events* (New York: Oxford, 1980).

[8] Cf. Wittgenstein's discussion in *The Blue Book* (New York: Harper & Row, 1958).

[9] "Intentional Systems," in *Brainstorms* (Montgomery, Vt.: Bradford Books, 1978), pp. 9, 11.

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committed to accepting, these truths. Also, a person can endorse a deductive system—for instance, as an object language for the relatively restricted technical purposes of metamathematics. The more limited the use of the system—the more it is preached as a norm on Sunday, but not practiced the rest of the week—the more such endorsement tends to fall below strong acceptance.

V. PRACTICAL ADEQUACY OF A LOGIC

The question of universal acceptance of logic now becomes, Must a minimally rational agent accept a logic either strongly or weakly? The interesting issue is whether an agent's satisfaction of the minimal rationality conditions implies his strong acceptance of a sound and complete deductive system. (The argument below can also be adapted for the weak sense of 'accept'.) The question needs further sharpening: One might argue[10] that, though an agent must be able to make some sound inferences—that is, must have some deductive ability—he does not have to be able to make any *particular* inferences, even those which normal human beings find the most obvious. But even if this is true, it does not exclude universal acceptance of logic. It might still turn out that any agent must strongly accept some complete set of valid laws and sound rules; it would just be that agents do not have to accept the laws and rules normal humans do—for instance, those normal humans find obvious. Our question is therefore, Must an agent strongly accept *any* set of valid laws or sound rules that constitute a complete deductive system, much less particular obvious laws or rules? It seems that an agent can have the deductive ability required by the rationality conditions without strongly accepting, i.e., actually using sometimes, even one such law or rule. (We restrict consideration to verbally formulated beliefs.)

We need one more distinction. I shall say that a deductive system is *metatheoretically* adequate if it is sound (and therefore consistent) and complete. In the first paragraph of "The Justification of Deduction", Michael Dummett asserts, "Failure of soundness yields a situation which must be remedied. Failure of completeness cannot always be remedied; a remedy is, however, mandatory whenever it is possible."[11] Dummett accurately describes adherence to

[10] As I have in my "Feasible Inferences," *Philosophy of Science*, XLVIII, 2 (June 1981): 248–268. See also sec. 5 of that paper for an argument against the claim that being able to make particular "obvious" inferences, by whatever means, is *constitutive* of understanding the logical constants involved in those inferences; the argument would apply in particular against the assertion that strong acceptance—that is, actual use—of the corresponding obvious valid laws or sound rules is required for an agent to qualify as understanding the logical constants involved.

[11] In *Truth and Other Enigmas* (Cambridge, Mass.: Harvard University Press, 1978), p. 290.

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such an absolute requirement as "the standard practice of logicians" in constructing and justifying a formal logical theory. However, the metatheoretic adequacy of a deductive system must be explicitly distinguished from its *practical* adequacy: here, its adequacy for accomplishing the deductive tasks required of a minimally rational agent. If one assumes that any possible agent must be ideally rational, it is easy to overlook the difference between the two types of adequacy. But with a minimal rationality model, the overlap of the two types of adequacy becomes much less salient.

What is the relation between metatheoretic and practical adequacy? I shall point out that practical adequacy does not require metatheoretic adequacy, that the former is sometimes preferable to the latter, and that the former may sometimes not even be compatible with the latter. The very quickest possible, but least reliable, way of performing a deductive task is just to guess the answer. We know that a minimal agent does not have to be a perfect logician, but the agent could not accomplish his required sound inferences (while avoiding enough unsound ones) just by a series of lucky guesses. There are, however, other ways to improve above chance the odds of selecting conclusions that follow from premises besides using a sound and complete deductive system. The agent might use what is in effect a better than random, but not perfect, gambling strategy for identifying sound inferences. Though such a rule of thumb would not always succeed, it might work sufficiently often to reach the break-even point of satisfying minimal rationality requirements. I shall argue later that this type of strategy may be indispensable if computational intractability is to be avoided.

VI. AGAINST METATHEORETIC ADEQUACY

The concept of such a strategy suggests, to begin with, that it is at least possible for a logically competent agent to have one or more "logical blind spots" which are the result of his exclusively using an incomplete deductive system (whenever he does use a deductive system). Given the difficulties for observer—and agent—in determining the agent's nonconscious cognitive processes, let us consider the overt and explicit steps written out by a person performing a formal derivation (or alternatively, the core dump of a computer running a theorem-proving program). As an uninteresting example, the agent might use a conventional textbook natural-deduction system of independent rules, with a *modus tollens* rule that has a clause that excludes its application to formal sentences with more than 1,000 logical constants. Similarly, it is possible for an agent to perform all required sound inferences by means of an unsound, or even inconsistent, system. Frege's axiomatization of logi-

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cal theory in *The Fundamental Laws of Arithmetic* and Quine's in *Mathematical Logic* were both inconsistent in ways that did not reveal themselves to many who had used each axiomatization extensively. These two examples suggest that first-order deductive systems can correspondingly be inconsistent in ways that do not yield too many—indeed, any—unsound inferences for the range of deductive tasks required of an agent. And in fact all the early formulations of the substitution rule for the predicate calculus are reported to have been unsound.[12] (Of course, if a deductive system is inconsistent it is complete, but this is no longer metatheoretic virtue.)

Furthermore, an agent who satisfied the minimal rationality condition could use exclusively a deductive system in which *all* axioms were invalid and *all* inference rules were unsound. A natural-deduction system corresponding to a standard textbook one, but composed entirely of unsound rules can easily, if uninterestingly, be constructed. For example, to the original *modus tollens* rule a clause is added: "When one of the premises contains more than 1,000 logical constants, the set of premise-numbers of the line on which the conclusion occurs should be empty; otherwise the premise-numbers are as usually specified." A similar premise-number clause can be added to each of the other rules. Or the original other rules and the new *modus tollens* rule can just be conjoined as a single rule; such a matter of individuating rules seems arbitrary.

The agent would not use the usual shortcut "theorem" rule which permits entering in a derivation a previously proved theorem with an empty set of premise-numbers. The claim here is just that this agent *can* use exclusively this set of unsound basic rules. The agent might happen to be uninterested in using that set to deduce "vacuous" valid sentences; perhaps, as empirical studies indicate for normal human beings, he has difficulty reasoning so abstractly. As proponents of the naturalness of natural-deduction systems often point out, outside of logic courses people rarely seem to use, or at least to cite, logical validities.

The unsound inferences permitted by this system would be performed relatively rarely because they would arise only under a restricted range of conditions: they involve very complex sentences, or might be otherwise unintuitive or difficult for the agent to perform. We know that an agent cannot perform all inferences—in particular, the more complex ones—anyway; so the unsoundness of this system need not detract *at all* from the agent's rationality. We

[12] S. Kleene, *op. cit.*, fn, p. 107. See A. Church, *Introduction to Mathematical Logic*, vol. 1 (Princeton, N.J.: University Press, 1956), pp. 289/90.

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conclude that metatheoretically adequate deductive systems are not the *only* way to achieve practical adequacy. One cannot argue that any possible rational agent must accept logic.

Furthermore, a stronger point against metatheoretic adequacy seems to hold: that in important cases it is antagonistic to practical adequacy. A metatheoretically inadequate system could be superior to any metatheoretically adequate one for the practical purposes of accomplishing an agent's everyday deductive tasks, just as inconsistent naive set theory is often more convenient than one of the consistent axiomatizations. In such a situation, insisting upon use of a metatheoretically adequate system would itself be unreasonable, like trying to use the more correct but hopelessly unwieldly relativity physics instead of classical mechanics for engineering calculations in designing a bridge. Even outside of practical contexts empirical theories are often recognized to be idealizations that are only approximations of reality and apply satisfactorily only over limited ranges of the parameters involved; the kinetic-molecular theory of gases is a standard example of an idealization that is employed because it is much more manageable than more correct theories, e.g., that do not assume molecules are perfectly dimensionless spheres.[13]

Indeed, as mentioned earlier, much evidence has recently emerged indicating that in a remarkably wide range of conditions human beings do not in fact use formally correct procedures in everyday nondeductive reasoning. And occasionally researchers in this field have pointed out, in effect, that use of such quick but dirty heuristics in "applied" as opposed to "pure" situations may be a reasonable speed-accuracy tradeoff.[14] There is also a separate tradition of empirical research suggesting that people do not use formally correct procedures in simple deductive reasoning. In addition, some of my own recent empirical studies indicate that subjects use a "prototypicality heuristic" in deductive reasoning, a set of shortcut strategies that exploit structuring of concepts in terms of prototypes, or best examples, of the concepts; we seem to extend the "context of discovery" in this way into the "context of proof." Furthermore, some of the evidence suggests that using this formally incorrect procedure is in fact rational, in that it pays off with lower error rates. These last findings are significant because they identify a connection between deductive-reasoning heuristics and the important recent research on prototype models of mental rep-

[13] The question of adopting convenient but in some sense unsound inference rules in fact sometimes is raised in devising natural-deduction systems for introductory logic texts; see, for example, Mates, *op. cit.*, p. vii.

[14] See, for example, the last chapter of Nisbett and Ross, *op. cit.*

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resentation.[15] Finally, it is worth recalling in this context the widespread occurrence of the simplest classical semantic and set-theoretic antinomies in our conceptual scheme, from the foundations of mathematics to ordinary discourse; this may be another symptom of our use of formally incorrect deductive procedures.

VII. PRACTICAL PARALYSIS

An even stronger point may hold than just that metatheoretically inadequate systems appear to be preferable sometimes to metatheoretically adequate ones. Some of the recent research on computational complexity raises the possibility that metatheoretic adequacy may in important ranges of cases be entirely incompatible with practical adequacy, both for some of the "pure" purposes of the deductive sciences and for the "applied" purposes of maintaining an agent's minimal rationality. Let us begin with an argument of Michael Rabin's[16] for the introduction of a notion of probabilistic proof in mathematics. Of course no decision procedure is possible even in principle for all of elementary number theory, but even in-principle decidability can sometimes be of very limited value. Consider a result of Albert Meyer and Larry Stockmeyer's[17]: Although the set of theorems of a formal system for the weak monadic second-order theory of successor (WS1S), a fragment of elementary number theory, is decidable in principle, its decidability seems extremely unfeasible in practice. The problem requires not just exponential time, but "super-exponential" time. To prove theorems of only 617 symbols or less would require a network with so many boolean elements that, even if each were the size of a proton, the machine would exceed the size of the entire known universe. In effect, the moral Rabin drew from the pattern of such complexity-theoretic results is that, to avoid the problem of unfea-

[15] An early (and controversial) study that suggested subjects use a kind of global impression or "atmosphere" of the logical form of the inference was R. Woodworth and S. Sells, "An Atmosphere Effect in Formal Syllogistic Reasoning," *Journal of Experimental Psychology*, XVIII (1935): 451–460; see also P. Wason and P. Johnson-Laird, *Psychology of Reasoning* (Cambridge, Mass.: Harvard, 1972), ch. 10. For a review of research on prototypicality, see E. Rosch, "Human Categorization," in N. Warren, ed., *Studies in Cross-cultural Psychology*, vol. 1 (New York: Academic Press, 1977). On the use of a prototypicality heuristic in deductive inference, see my "Prototypicality and Deductive Reasoning," *Journal of Verbal Learning and Verbal Behavior* (October 1984).

[16] See "Theoretical Impediments to Artificial Intelligence," in J. Rosenfeld, ed., *Information Processing 74* (Amsterdam: North Holland, 1974). On Rabin's concept of a probabilistic algorithm, see his "Probabilistic Algorithms," in J. Traub, ed., *Algorithms and Complexity* (New York: Academic Press, 1976). Rabin's argument is described in G. Kolata, "Mathematical Proofs: The Genesis of Reasonable Doubt," *Science*, CXCII (1976): 989/90.

[17] See A. Meyer, "Weak Monadic Second-order Theory of Successor Is Not Elementary-Recursive," in A. Dold and B. Eckmann, eds., *Lecture Notes in Mathematics*, no. 453 (New York: Springer-Verlag, 1975); and Stockmeyer and Chandra, *op. cit.*

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sibly long proofs, mathematicians sometimes should make the ultimate speed-accuracy tradeoff: relax the metatheoretic requirement of consistency, even where it is in principle satisfiable, to evade practical paralysis. Rabin recommended, and devised, methods of probabilistic proof which do not guarantee truth, but for which the probability of error can be determined to be, e.g., one in a billion.[18]

Rabin's strategy might be compared with undoing Descartes's bargain; Descartes sought apodeictic certainty, but the cost is recognized to have been epistemic paralysis. We can extend the point from methodology of the deductive sciences to our concern about fundamental constraints on human cognition. There are now two possible extremes for dealing with, e.g., the problem of determining tautological consequence. Just guessing, the quickest but dirtiest procedure, is too dirty for even minimal rationality, since odds of success are chance. The other extreme, a decision procedure, is the most reliable but also seems too slow; it is perfectly infallible, but, as noted earlier, it is probably computationally intractable, which might be too slow for even minimal rationality. Therefore, a compromise between the two extremes seems needed to yield sufficient deductive capacity for minimal rationality

Various tradeoff strategies are in fact prevalent in computer science in dealing with problems that are found to be computationally intractable. Standards are lowered and "heuristic algorithms" and "approximation algorithms" are sought for the problems instead of perfect optimization algorithms.[19] There is more than one type of compromise with perfect algorithmhood which might evade the apparent intractability of decision procedures for even just tautologous consequence. One might, for instance, use a metatheoretically adequate deductive system, but avoid worst cases by restricting its application to simpler special cases—sets of premises and a possible conclusion that are small enough so that the exponential explosion of operations is not severe. Therefore, the agent could never attempt to make an inference from, or to test for consistency, his entire belief set, or even a large portion of it. The cost, and the eventual limitation, is that the agent would then exhibit the most rationality "locally", within certain neighborhoods of his beliefs, and would be particularly weak on inferences and consistency involving beliefs distributed between such subsets. (In fact, a fundamental feature of human belief systems is that they are

[18] Hilary Putnam has also argued, in a Quinean vein, that "quasi-empirical" methods—resembling those employed in evaluating the plausibility, for instance, of highly theoretical statements in physics—have always been important in mathematics outside of the domain of formal proof. See "What Is Mathematical Truth?", in *Philosophical Papers*, vol. 1 (New York: Cambridge, 1975).

[19] See, e.g., Garey and Johnson, *op. cit.*, ch. 6.

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"compartmentalized" in this way; thus, an important rationale for this structuring is as a strategy for evading intractability.[20]) Another, simpler strategy would be: Given any case, employ the metatheoretically adequate system; if no answer results within some fixed time limit, just abandon the attempt and flip a coin to pick an answer.

VIII. REAL-WORLD RELEVANCE

These strategies, however, require that computationally manageable cases involving sufficiently large belief sets be sufficiently frequent to avoid *de facto* computational paralysis. We must therefore turn to the issue of the "real-world relevance" of complexity theory. It should be emphasized that usual complexity measures are not average-case estimates. The typical theorem stating that a problem is computationally complex is of a worst-case form: Given any algorithm for deciding each instance of the problem, each of an infinite number of cases requires exponential time.

This still leaves in limbo an infinite number of *other* cases of the problem. Which of them, if any, requires exponential time? Perhaps every instance of the problem takes exponential time; even then, the exponential blowup might be so slow that the computational cost is not severe for small cases of the problem. Or instead, perhaps no real-world relevant case—of less than colossal size—might require "serious" exponential time; the exponential cases might be of an input size that nothing of human-scale computational resources could ever even encounter. Or again, the problem's complexity profile might fall messily between these two extremes. Therefore information is needed on the "density", or population distribution, of the hard cases. This includes, for example: (a) Do they arise "early" (for cases of about the same order of size as the shortest decision algorithm for the problem)? (b) Do they arise "often", that is, within the population of *interesting* cases; this requires an understanding of which instances are interesting, which of course will be relative to particular goals. (c) How severe is the exponential explosion, when it does occur?

The probability distribution of relevant worst cases is presently not well understood.[21] Indeed, algorithms for linear programming

[20] See my "Rationality and the Structure of Human Memory," *Synthese*, LVII, 2 (November 1983): 163–186.

[21] For an investigation of probability-distribution, as opposed to worst-case, analyses, see R. Karp, "Probabilistic Analysis of Some Combinatorial Search Algorithms," in J. Traub, ed., *op. cit.* Also relevant for the satisfiability problem is S. Mahaney, "Sparse Complete Sets for NP: Solution of a Conjecture by Berman and Hartmanis," *21st IEEE Symposium on Foundations of Computer Science* (1980): 54–60.

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are a well-known counterexample to the assertion: "An algorithm is in fact practically unfeasible if and only if it requires exponential time." On the one hand, the Simplex linear programming algorithm has been proved to require exponential time. Yet for decades the Simplex algorithm has been found very usable in practice—for the population of problems of interest to its users. A recent "empirical" study of running times of the algorithm for actual problems in the banking, steel, and oil industries confirms this; and Steven Smale has just proved that the exponential cases are in a sense rare. On the other hand, the "Khachian" algorithm requires only polynomial time, but its typical running time seems much worse than that of the Simplex algorithm, because its polynomial bounds are so high.[22] The connection of exponential time with in-practice unfeasibility thus needs to be interpreted with some care.

Nonetheless, workers in many areas of computer science certainly continue to accept this connection as a very useful rule of thumb.[23] And some rough estimates do not suggest optimism that methods like the above compartmentalizing and "give up and guess" strategies are by themselves sufficient to avoid intractability in the management of a human belief system. Even decidability of just the monadic predicate calculus (and of some other decidable subclasses of the full predicate calculus) is known to require nondeterministic exponential time.[24]

And, at least as food for thought, it is worth again considering testing for tautological consequence, only a very small part of the general problem here, by means now of the truth-table method (even the more efficient known test procedures such as Wang's algorithm or the resolution method still require as much time in the worst cases). Given the difficulties in individuating beliefs, it is not easy to estimate the number of logically independent atomic propositions in a typical human belief system, but 138 seems much

[22] For the proof that the Simplex algorithm requires exponential time, see V. Klee and G. Minty, "How Good Is the Simplex Algorithm?" in O. Shisha, ed., *Inequalities-III* (New York: Academic Press, 1972). The "empirical" study of running times is in E. McCall, "Performance Results of the Simplex Algorithm for a Set of Real-world Linear Programming Models," *Communications of the Association for Computing Machinery*, XXV (1982): 207–212. Smale's proof has been reported in *Science*, CCXVII (1982): 39. A short review of the so-called "Khachian" algorithmn: L. Lovacs, "A New Linear Programming Algorithm—Better or Worse than the Simplex Method?" *Mathematical Intelligencer*, II (1980).

[23] See, for example, pp. 8/9, Garey and Johnson, *op. cit.*

[24] For a review of these results, see H. Lewis, "Complexity of Solvable Cases of the Decision Problem for the Predicate Calculus," *Proceedings of the 19th IEEE Symposium on Foundations of Computer Science* (1978): 35–47.

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too low—too "small-minded". Yet suppose that each line of the truth table for the conjunction of all these beliefs can be checked in the time a light ray takes to traverse the diameter of a proton, an appropriate cycle time for an ideal computer. At this maximum speed, a consistency test of this very modest belief system would require more time than the estimated twenty billion years from the dawn of the universe to the present. Quinean or Davidsonian charity requirements that a translation be readjusted if it yields an inconsistent belief set seem particularly unrealistic in this light.

Furthermore, it is important to note that some exponential-time problems have been proved to have hard cases of small size (indeed, similar proofs have been emerging for classical absolutely unsolvable problems[25]). As noted above, deciding WS1S sentences of just several hundred symbols is known to require more space (and time) than there is in the known universe. Also, Fischer and Rabin have showed that Presburger arithmetic requires exponential time and, furthermore, that the exponential explosion of proof length sets in early, for sentences of the same order of size as the decision algorithm.[26]

Nonetheless, it is quite easy to show that an early onset of computational complexity—that is, for cases small enough to be humanly relevant—is not an inherent feature of intractability: Given any intractable problem with early-onset complexity, one can always construct another problem with complexity onset only for cases larger than a particular given size.[27] The significant implication here of this point is that "counting the horse's teeth"—empirically observing which are the difficult and interesting cases of a

[25] Undecidability for elementary arithmetic is now known not to arise only for extremely complex and mathematically uninteresting sentences: See J. Jones, "Three Universal Representations of Recursively Enumerable Sets," *Journal of Symbolic Logic*, XLIII (1978): 335–351, for an undecidable, unabbreviated sentence (based on the solution of Hilbert's Tenth Problem) of about 100 symbols in length; and his "Universal Diophantine Equation," *Journal of Symbolic Logic*, XLVII, 3 (September 1982): 549–571, for the basis for constructing still shorter sentences. See also G. Chaitin, "Information-theoretic Limitations of Formal Systems," *Journal of the Association for Computing Machinery*, XXI (1974): 403–424. In addition, a "mathematically simple and interesting" theorem (an extension of the finite Ramsey theorem) is not provable in Peano arithmetic; see J. Paris and L. Harrington, "A Mathematical Incompleteness in Peano Arithmetic," in J. Barwise, ed., *Handbook of Mathematical Logic* (New York: North-Holland, 1977). The present paper further motivates study of the "density", or distribution, of undecidable sentences.

[26] M. Fischer and M. Rabin, "Super-exponential Complexity of Presburger Arithmetic," in *Complexity of Computation, SIAM-AMS Proceedings*, VII (1974): 27–41.

[27] For example, as noted, the set of theorems of Presburger arithmetic has early-onset complexity. However, the set of Presburger arithmetic theorems, each of length more than 10,000 symbols, can be quickly decided for any case of length less than 10,000 symbols; the algorithm will just count the number of symbols in any sentence to be decided and immediately reject it if that length is less than 10,000.—Another question concerns the "naturalness" of such truncated problems.

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problem—will often be unavoidable; much of the "real-world" complexity structure of many intractable problems is at least presently a hybrid question, to be approached in the manner of McCall's study of the Simplex algorithm cited earlier. In particular, artificial intelligence workers on automatic theorem proving have uniquely valuable data about what are the "interesting", frequently occurring, cases of a given intractable problem, and which of them in fact require large expenditure of resources. And similar information can correspondingly be obtained for human deductive reasoning. That is, in the hypothetico-deductive manner, we can treat the issue of whether there is real-world relevant complexity at least as an empirical working hypothesis, suppose it is true, and see how well its implications are supported by observations.

In fact, the rather wide range of recent evidence mentioned earlier confirms that people actually do not use formally correct methods in their intuitive reasoning. Instead, they seem to use procedures in nondeductive everyday reasoning such as Kahneman and Tversky's "representativeness" and "availability" heuristics, and related "prototypicality heuristics" in deductive reasoning. Thus, the most fundamental "empirical" suggestion of complexity theory, that algorithmic methods of accomplishing even some very simple deductive tasks are likely to be intractable, would provide a unifying framework for explaining why we use such heuristics instead. It constitutes the ultimate "justificaton of the ways of Man" here. For a plausible conjecture to begin with is that our quick and dirty shortcut strategies are required to avoid intractability. To the constraint that information-processing models of cognition should be finitary, we could then add that, in at least some interesting cases, they also should not entail certain classes of computationally complex processes. The possibility needs to be explored further that, to a considerable extent, the only way human reasoning (or any agent's) can evade practical paralysis is by not using metatheoretically adequate deductive systems.[28] Ideal rationality requirements seem to exclude even entertaining such a possibility as more than unimportant exceptions to a rule, but a theory of minimal rationality provides a principled basis for relating complexity theory and the psychological studies in this way.

[28] Correspondingly in artificial intelligence, the algorithmic approach of seeking classical decision procedures, or even just complete proof procedures, for theoremhood needs to be reevaluated in light of the problem of combinatorial explosion of branchings in proofs. For a brief discussion of the issue, see A. Newell and H. Simon, "Computer Science as Empirical Inquiry," *Communications of the Association for Computing Machinery*, XIX (1976): 113–126. See also Rabin, "Theoretical Impediments to Artificial Intelligence," *op. cit.* An extensive review of practical limitations on theorem-proving programs (and program verifiers) still is needed.

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The familiar debate about how to choose between competing logics, e.g., which is more "natural", presupposes as an absolute requirement that we ought not even to consider a logic that is metatheoretically inadequate. But we can now see that the commonplace that Dummett, for example, began with, that metatheoretic adequacy is mandatory whenever "in principle" possible, needs at least to be carefully restricted. If one is engaged in a metatheoretical investigation of a deductive system, rather than generally using the system, Dummett's assertion may be correct. An agent can endorse the use of a metatheoretically adequate system, reason *about* it, and even really use it in some limited contexts. But if the system is actually to be used significantly, metatheoretic inadequacy may be reparable only on pain of intractability; it would then be irrational even to try to adopt exclusively a metatheoretically adequate system, since that would preclude successful reasoning. It would be like insisting upon the perfectionism of the Cartesian methodology of universal doubt, with its resulting cognitive paralysis.

Thus, in contrast to Alfred Tarski's remark, "the appearance of an antinomy is for me a symptom of disease,"[29] there is at least some truth to Wittgenstein's earlier image in his account of the (non)significance of the paradoxes, "a contradiction is not a germ which shows a general illness."[30] The contradiction need not *entirely* vitiate the system. Indeed, this paper has gone further, proposing that such inconsistency may be downright healthy. The moral regarding the thesis of the universal acceptance of logic is that once we begin to take into account at least the most fundamental facts of an agent's psychological reality, that thesis, like a number of other rationality idealizations, seems wrong, and interestingly so. Contrary to the usual charity principles, not only is acceptance of a metatheoretically adequate deductive system not transcendentally indispensable for an agent's rationality, but in important cases it is inadvisable and perhaps even incompatible with that rationality.

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[29] "Truth and Proof," *Scientific American*, CCXX (1969): 63–77.
[30] *Wittgenstein's Lectures on the Foundations of Mathematics: Cambridge, 1939*, C. Diamond, ed. (Ithaca, N.Y.: Cornell, 1976), p. 211.

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KRIPKE'S ACCOUNT OF THE ARGUMENT AGAINST PRIVATE LANGUAGE*

SAUL Kripke's *Wittgenstein on Rules and Private Language*† suggests interpreting Wittgenstein's argument against private language as a direct corollary of the considerations about rule following which immediately precede those passages in the *Investigations* (§243 and following) on which more traditional attempts to understand Wittgenstein's thought on privacy have tended to concentrate. For a long time I thought Kripke's interpretation of these matters more or less coincident with that at which I had arrived independently and which I had presented in my *Wittgenstein on the Foundations of Mathematics* and elsewhere.[1] A careful reading of Kripke's book has convinced me both that this is not the case and that the dominant impression given in my book of the relation between the private-language argument (PLA) and the rule-following considerations (RFC) is misleading.

The leading suggestion about the PLA in that book was that it is to be viewed as *part* of the considerations about rule following: an argument, essentially, that the sort of objectivity of meaning necessary if we are to think of the truth values of unconsidered, uninvestigated statements as determinate independently of any investigation we may carry out, can find no refuge in the situation of a single speaker and his idiolect. The RFC were then depicted as taking the argument outwards, as it were—arguing, first, that, within the sphere of communal practice, concepts and distinctions can be given currency, on the basis of which a "thinner" notion of correctness and incorrectness in linguistic usage can be rehabilitated than that sanctioned by objective meaning; but, second, that, as far as the propriety of objective meaning is concerned, the community at large ultimately fares no better than the would-be private linguist. With none of this, at least as a potentially fruitful framework for the investigation of Wittgenstein's later philosophies of mind and mathematics, do I now disagree. But it does seem to me now that the treatment in my book could be usefully supplemented in at least two respects.

*This paper amplifies part of the discussion of my "Kripke's Wittgenstein," presented at the 7th International Wittgenstein Symposium in Kirchberg-am-Wechsel, Austria, in 1982, but omitted from the associated published volume.

†Oxford: Basil Blackwell, 1981. All page references are to this text unless otherwise stated.

[1] London: Duckworth, 1980. See also C. Leich and O. Holtzman, eds., *Wittgenstein: To Follow a Rule* (London: Routledge & Keagan Paul, 1981), pp. 99–106; and "Strict Finitism," *Synthese*, LI, 2 (May 1982): 203–282, pp. 248–252.

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First, I think the involvement of "antirealist" premises in the arguments against objective meaning was there overemphasized; it seems to me that a more sensitive, sparing, and concept-specific use of such premises may be possible without compromise of the power of the argument. Second, more stress is wanted that Wittgenstein has a *differential* claim about private language: that the would-be private linguist and the community are not, in the end, in the same predicament. I shall not, in this paper, attempt to enlarge upon either of these claims.[2] The notable point is that an analogue of each is a prominent feature of Kripke's interpretation: the *skeptical argument* needs no anti-realist (verificationist) assistance; and the bearing of the *skeptical solution* on private language admits of no community-wide generalization.

Although I do not think that Kripke has Wittgenstein right, my subject, except in the last section of the paper, is not the historical Wittgenstein but Kripke's Wittgenstein. I shall argue that, even if the main argument—the skeptical argument—which Kripke finds in Wittgenstein, is sustained, there is strong prima facie reason to doubt whether the accommodation with it—skeptical solution—which Kripke represents Wittgenstein as commending can really be lived with; whether, indeed, that accommodation is so much as coherent. And I shall canvass ways, unconsidered (or only very cursorily considered) by Kripke, for resisting the skeptical argument. The upshot will be that the RFC, as interpreted by Kripke, are flawed by a lacuna, and that, even if the lacuna were filled, the PLA could nevertheless not emerge in the manner that Kripke describes.

Because the gist of my remarks about Kripke's book is going to be largely critical, it is perhaps worth emphasizing my admiration of it. Whatever its relation to Wittgenstein's actual thought, and whether or not ultimately cogent, Kripke's dialectic is tremendously exciting. It will surely provide a great spur to improving our understanding of Wittgenstein's philosophy.

I. THE SKEPTICAL ARGUMENT

Fundamental to Hume's moral philosophy, as to his views about causation, is a distinction between statements that are apt to express real matters of fact and certain sentences that, although possessed of standard features of the syntax of genuine statements—in

[2] They are enlarged on in, respectively, "Rule-following and Constructivism," in C. Travis and B. Gelder, eds., *Inference and Understanding*, projected, and "Does Wittgenstein Have a Cogent Argument against Private Language?" in J. McDowell and P. Pettit, eds. *Context, Content and Thought*, forthcoming (New York: Oxford, 1985).

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particular, the capacity to serve as arguments for various types of statement-forming operator—are actually used not to state facts but rather to *project* various aspects of speakers' attitudes and affective responses. Moral discourse, and talk of causation, belong, for Hume, in the latter category. Moral judgments, so viewed, do not express our cognition of moral facts by which various moral sentiments in us are generated; rather they serve to project those moral sentiments upon the world. Likewise, those statements which the non-Humean takes to aver the existence of causal relations, from which certain observed regularities flow, serve for Hume to project an attitude that we take up toward those observed regularities.

It is familiar that this sort of distinction is pivotal to a whole class of important philosophical disputes. Realism not merely about ethics and causation, but also about aesthetics, theoretical science, pure mathematics, logical necessity, and Lockean secondary qualities may each be opposed by appropriate versions of Humean noncognitive "projectivism."

In this light, Kripke's Wittgenstein may be seen as first, by the skeptical argument, confounding the ordinary idea that our talk of meaning and understanding and cognate concepts has a genuinely factual subject matter, and then, via the skeptical solution, recommending an alternative projective view of its content. It is worth emphasis that the skeptical solution is independent of the skeptical argument: strictly, the option is open of simply accepting the latter as demonstrating the vacuity of all our talk of meaning, etc., with no prospect of its rehabilitation. (Similarly, a sympathizer might regard Hume as having demonstrated that we should simply drop all talk of causation.) Accordingly, it might seem as though one way of resisting the PLA, as Kripke interprets it, would simply be to take the skeptical medicine straight, eschewing the compensating sweetmeat of the skeptical solution afterwards. I shall return to that thought.

There are a variety of ways in which it might be argued that a region of discourse apparently apt for the stating of facts does not really perform that role. One way, the Humean strategy, would be to argue that from within the framework of a certain preferred epistemology, no reputable conception can be attained of the putative species of fact in question. Another general strategy would be to let the argument flow from a topic-neutral account of the ways in which the distinction between fact-stating and non-fact-stating declarative sentences comes out in their respective modes of employment in the language. But the strategy of argument which Kripke finds in Wittgenstein is different from both of these. Roughly, the

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conclusion that there are no facts of a disputed species is to follow from an argument to the effect that, even if we imagine our abilities idealized to the point where, if there were such facts to be known, we should certainly be in possession of them, we *still* would not be in a position to justify any particular claim about their character. So we first, as it were, plot the area in which the facts in question would have to be found if they existed and then imagine a suitable idealization, with respect to that area, of our knowledge-acquiring powers; if it then transpires that any particular claim about those facts still proves resistant to all justification, there is no alternative to concluding that the "facts" never existed in the first place.

The initial target class of putative facts comprises those which you might try to express by claims of the form "By *E*, I formerly meant so-and-so." The relevant idealization will involve your total recall of all facts about your previous behavior and previous mental history, it being assumed that facts about your former meanings must be located in one of those two areas if they are located anywhere. The argument will then be that, even in terms of the idealization, no such claim is justifiable. It follows that your previous life in its entirety is empty of such facts, and hence that there are none (cf. 21 and 39).

I have sometimes encountered in discussion the complaint that, whatever independent force Kripke's development of the argument may have, its use of skepticism betrays its claim to represent Wittgenstein's actual thought. In one way, this misconstrues the skeptical argument; in another way, however, it may have a point. The misunderstanding consists in a failure to see that Kripke's skeptic is a mere device, annexed to the demonstration of a projectivist thesis which might well be supported in other ways. (It is notable that the historical Wittgenstein, though undoubtedly hostile to classical forms of skepticism, unmistakably displays projectivist leanings in certain of his remarks, e.g., on first-person ascriptions of sensation and when he compares mathematical statements to rules.) Classical forms of skepticism purport to discover inadequacies in our *actual* cognitive powers: the skeptic about induction, or other minds, or memory, holds that the best we can do, in attempting to arrive at justified opinions concerning statements in the relevant classes, always falls short of anything that ought really to be counted as justification. The skeptic whom Kripke finds in Wittgenstein, in contrast, is concerned to teach us something about the range of items that *exist to be known*. That said, there will still be a point to the complaint if it turns out that, despite these differences, the *techniques*

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utilized by Kripke's skeptic are importantly similar to those which feature in traditional skeptical arguments. Wittgenstein undoubtedly thought those arguments mistaken; it is hardly likely that he would have allowed himself to succumb to an argument which, even if tending toward a conclusion congenial to him, needed to rely upon epistemological principles that, if granted, would enormously strengthen the traditional skeptic's case. We shall consider the matter in due course.

Suppose it granted that there are indeed no facts that we can express by statements of the form, "By *E*, I formerly meant so-and-so." How exactly do destructive consequences follow about the notions of meaning and understanding in general? Kripke himself is fairly brief on the point (13), but it is not difficult to see. Remember that the argument will have involved an extensive idealization of your knowledge of your previous behavior and mental history: you will have been granted perfect recall of all such facts. If it turned out that you still could not justify any preferred claim of the form "By *E*, I formerly meant so-and-so," then how can you be better placed to justify a claim of the form "By *E*, I presently mean so-and-so"? For anything true of your mental life and behavior up to and including the present will be known to you tomorrow, in accordance with the idealization. And the argument will have shown that tomorrow you won't be able to justify any claim of the form "By *E*, I yesterday meant so-and-so." Hence you cannot be in a position to justify the present-tense counterpart of that claim today. The idealization also entails that nobody else is better placed than you to justify any such claim. It follows that nobody can justify any claim about what they, or anybody else, formerly meant or means. Hence, in the presence of the idealization, there can be no facts about what anybody means by any expression. And it is impossible to see how, consistently with that admission, there might yet be facts about what expressions, as it were impersonally, mean. The strategy of the skeptical argument thus appears sound and ingenious. Everything depends upon the details of its execution.

Simplifying the details somewhat, the execution runs essentially like this. Suppose you claim, on December 21 1984, that by 'green' you meant, on December 20 1984, *green*. The skeptic challenges you to justify your claim. You are idealized to have perfect recall of all your previous linguistic and nonlinguistic behavior, together with your entire mental life—the whole pageant of your thoughts, sensations, imaginings, dreams, moods, etc. (At this point in the argument there is, of course, no doubt entertained as yet about the supposition that you *presently* know what you mean by 'green'—

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the skeptic gets you to stand on the rug before he pulls it away; cf. 11/2.) Now no doubt you can cite a lot of behavior that is broadly consistent with your preferred account of your former understanding of 'green'. The skeptic will point out, however, that there are no end of alternative interpretations of your former meaning, all of which rationalize your behavior equally well. Perhaps, for example, by 'green' you formerly mean $grue_{1984}$; where an object is $grue_t$ at time k just in case k is earlier than the inception of January 1 in year t and the object is green, *or* k is some later time and the object is blue. Seemingly there are indefinitely many grue-interpretations that can be used to make sense of your previous applications of 'green', all of which are incompatible both with the supposition that by 'green' you formerly meant green and with each other.

It might be objected that this is to consider only one kind of use of 'green', viz., simple ascriptions and withholdings, and that account will need to be taken of all sorts of other more sophisticated uses, including embeddings in descriptions of your own and others' propositional attitudes, which you previously will likely have made. But, of course, the point of the example is that 'green' will be assigned the same extension, up to and including the time of your dialogue with the skeptic, under indefinitely many grue-interpretations; so the skeptic should have no difficulty in rationalizing your previous uses both in extensional contexts and in all attributions of propositional attitudes whose possession, by yourself and others, can be explained in terms of the extension of the property of being green. And even if grue-interpretations don't work out in general, the decisive consideration is surely that your previous behavior with 'green' is *finite*; hence, it must be possible, it appears, with sufficient ingenuity, to come up with some interpretation of your previous understanding of 'green' which will be as unwelcome to you as the grue-interpretations. Finite behavior cannot constrain its interpretation to within uniqueness.

The arena of battle now shifts to the mental. Perhaps considerations uniquely determining your previous understanding of 'green' can be recovered from your previous thoughts, imaginings, etc. It is evident, however, that, if the search is to succeed, the relevant mental items must have a certain *generality*. It is no good remembering imagining certain green things, or green after-images that you may have experienced, or thoughts about what you would have said if asked to describe the color of that liqueur we had on such-and-such an occasion. For the constraints imposed by introducing such considerations cannot be stronger than if the images had been public objects or if the imaginary and hypothetical situations had actually

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taken place; and the effect of those transformations would merely be finitely to enlarge an inadequately finite pool of actual data. You have to come up with some mental episode that somehow has sufficient content to exclude *all* the unwanted interpretations of your former understanding of 'green', including all the grue-interpretations, at one go.

The only candidate, it appears, is some sort of general *thought*: you need to have entertained a thought that has something to say about each of the situations in which the difference between the true interpretation of your former understanding of 'green' and each of the successive grue-impostors successively comes to light. On the face of it, though, it is not far-fetched to suppose that you might very well have entertained such a thought. What if you remember having thought, say, "By 'green' I certainly don't mean any concept which, at some particular time, will continue to apply to an object only if that object changes color at that time." Does not that at least force the skeptic to work a bit harder at the concoction of unwelcome interpretations?

It wouldn't be all that satisfactory if this were the best you could do. After all, it is pretty much fortuitous whether any such thought ever occurred to you; and your knowledge about your former understanding of 'green' will not seem to you contingent on such an occurrence. What you will want to be able to say is that you know what you formerly meant by 'green' *whether or not* you happen to have had a convenient thought that can be used to scotch a particular line of unwelcome interpretation. But the skeptic, in any case, has a stronger, indeed a seemingly decisive reply. His challenge, after all, was general. No special interest attaches to the justifiability of claims about your former understanding of color predicates: the question was, can *any* claim of the form "By *E*, I formerly meant so-and-so" be justified? Clearly the challenge is not met if, in the attempt to justify one such claim, you presuppose your right to be sure of another. But just such a presupposition is made by the attempted play with the general thought above. If, for example, by 'color' you had previously meant *schmolor* (20), where the concept of schmolor stands to all grue-type concepts exactly as color stands to blue, green, etc., then your having entertained that general thought is quite consistent with the skeptic's being correct in interpreting you as having meant grue_{1984} by 'green'.

The point is perfectly general. Thoughts you may have had about how, quite generally, you should be prepared to use an expression will suffice to meet the skeptic's challenge only if you presuppose their *proper interpretation*. But that is just to presuppose

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that the skeptic's challenge can be met with respect to the expressions that figure in those thoughts. Yet no category of mental item can be appropriate to the challenge except a general thought; only such a thought can have enough to say, can cover the indefinitely many potential situations in which you should wish to regard some determinate use of 'green' as mandated by the understanding that you believe you have long possessed of that expression.

It, therefore, appears that the only ploy that has any chance of accrediting your understanding of 'green' with an appropriately general normative role (11, 23, 24) totally fails to meet the skeptic's challenge. And now "it seems the whole idea of meaning vanishes into thin air" (22).

II. THE SKEPTICAL SOLUTION

Suppose that the skeptical argument is sound. Could we simply accept its conclusion and abandon all talk of meaning and understanding as founded upon error? Or must we seek some sort of rehabilitation of the concept of meaning, a skeptical solution? Kripke himself writes:

> . . . I choose to be so bold as to say: Wittgenstein holds, with the skeptic, that there is no fact as to whether I mean [green or grue$_t$]. But if this is to be conceded to the skeptic, is this not the end of the matter? What *can* be said on behalf of our ordinary attributions of meaningful language to ourselves and to others? Has not the incredible and self-defeating conclusion, that all language is meaningless, already been drawn? (70/1)

There is, however, a certain awkwardness here. Suppose someone runs a similar skeptical argument about moral obligation, concluding that statements about what people morally ought, or ought not, to do, lack a factual subject matter. It would be, to say the least, an infelicitous expression of this result to say, "So undertaking, and refraining from, any particular projected course of action are always both morally permissible." For the conclusion of the argument would apply equally to judgments of moral permissibility: to claim that a course of action is morally permissible is just to say that it is not the case that it ought to be refrained from. It is natural to wonder, correspondingly, whether the conclusion of Kripke's skeptic is indeed "incredible and self-defeating" only if the notion of meaninglessness which Kripke uses in its formulation presupposes the notion of meaning as moral permissibility presupposes the notion of obligation. If that is so, the right conclusion is surely that such is not the way to formulate the conclusion of the skeptical argument. Once it is better formulated, such un-

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happy claims as that all language is meaningless, that nobody ever succeeds in understanding anybody else, etc., will presumably not be entailed.

If somebody wishes to reject the suitability of a certain class of concepts to figure in statements apt to be genuinely true or false, this rejection cannot coherently take the form, it appears, of *any* kind of denial of statements in which those concepts figure. What then *is* the proper way of formulating the conclusions of Kripke's skeptic? One influential view of the concepts of meaning and understanding, associated chiefly with the writings of W. V. Quine, is that their paramount function for us is as theoretical terms in a deeply entrenched but philosophically suspect scheme of explanation of human linguistic behavior and of nonlinguistic but language-related patterns of social activity. If we think of this scheme as issuing in a large class of only semi-articulated theories about particular individuals and groups of individuals, then one way of expressing the conclusion of the skeptical argument is that *scientific realism* about these theories is not an option; there are no facts, describable only by recourse to the concepts of meaning and understanding, which such theories might succeed in codifying. An immediate consequence of this perspective is that two quite different lines of response to the skeptical argument are apparently open. One is a kind of *instrumentalism*: a view which tries to retain the propriety of theorizing of the sort in question while granting its nonfactual status. The skeptical solution attempts just this. The other response is to regard theorizing of this sort as *discredited*, and to seek better approaches involving quite alternative systems of concepts. That, in general, is Quine's own response to the difficulties that he finds in meaning and other intentional notions. If it admits satisfactory development, then the skeptical solution—and with it Kripke's reading of the PLA—would seem to be *de trop*.

This picture of the role of the concepts of meaning and understanding in our ordinary thinking is, however, an oversimplification. It ignores the larger class of self-directed statements concerning meaning and understanding which we make—the class that Wittgenstein himself gives special attention to—and, still more importantly, it makes nothing of various platitudes that articulate our conception of the connections between meaning and truth. One such platitude is that the truth value of a statement depends only upon its meaning and the state of the world in relevant respects. Equivalently:

> An utterance of *S* expresses a truth in a particular context if and only if what, in that context, *S* says is so, is so.

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The obvious corollary is that, if we take the view that the skeptical argument discredits *all* talk of meaning, understanding, and cognate concepts—like the concept of what a sentence is used to say—it is not clear how much purchase we can retain on our ordinary notion of a statement's being true. A proponent of the Quinean view has the choice either to abandon the notion of truth altogether or to reconstruct it in a fashion that liberates it from conceptual ties with the discredited notion of meaning. The former course, however, is hardly an option unless we are prepared to abandon the idea that it is *ever* the case that language has a fact-stating function. (And if that were our view, why see the conclusion of the skeptical argument as calling into question the propriety of talk involving the concepts of meaning and understanding?) The reconstructive project, on the other hand, looks to be utterly daunting. (Indeed it is doubtful whether, in the present context, it is coherent to suppose that there can be such a project; for whatever reconstruction of truth, free of all play with meaning and cognate notions, were proposed, it is not clear why an analogue of the skeptical argument would not be available to rob any particular assignment of truth conditions to a sentence of all possible behavioral or psychological corroboration.)

So the strategy incorporated in the skeptical solution may seem more attractive. It is in any case more comfortable to think of any errors involved in our talk of meaning, or our moral language, as *philosophical*: as belonging to our picture of what is going on in those areas of linguistic practice, rather than as undermining the practices themselves. Thus, Kripke suggests (73 ff) that statements involving the notion of meaning have no *truth conditions*, properly so described, but only conditions of justified or warranted use:

> All that is needed to legitimize assertions that someone means something is that there be roughly specified circumstances under which they are legitimately assertable, and that the game of asserting them under such conditions has a role in our lives (77/8).

Kripke's interpretation of the PLA now follows elegantly from this reorientation. Without attempting to do justice to the detail of his exposition (81 ff, summarized 107/8), we find that the most natural account of the justification conditions of statement forms like:

(i) Jones means addition by '+'.

and

(ii) If Jones means addition by '+', then he will answer '125' when asked, "What is 47 + 78?"

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involves essential reference to a community of practitioners with the symbols they mention. Very roughly, (i) will be considered justified if Jones performs satisfactorily often enough with '+' and marks his acceptance into the community of '+' users—those whose uses of '+' can generally be depended upon. And (ii) expresses a test for membership in that community, ratified by the responses of those already accredited with membership. Accordingly, such statement forms simply have no legitimate application to symbols whose use is essentially "private" and which cannot, in the nature of the case, competently be taken up by a community. So the concepts of meaning and understanding have no proper place in the description of an apparent linguistic practice of an individual, if that practice is one in which others could not competently share.

The elegance of Kripke's interpretation does not, however, long conceal its difficulties. One immediate difficulty is presented by the meaning-truth platitude. If the truth value of *S* is determined by its meaning and the state of the world in relevant respects, then nonfactuality in one of the determinants can be expected to induce nonfactuality in the outcome. (A rough parallel: If among the determinants of whether it is worth while going to see a certain exhibition is how well presented the leading exhibits are, then, if questions of good presentation are not considered to be entirely factual, neither is the matter of whether it is worth while going to see the exhibition.) A projectivist view of meaning is thus, it appears, going to enjoin a projectivist view of what is for a statement to be true. Whence, unless it is, mysteriously, possible for a projective statement to sustain a biconditional with a genuinely factual statement, the disquotational schema '$\ulcorner P \urcorner$' is true if and only if *P*' will churn out the result that *all* statements are projective.

Kripke's own remarks are confusing in this regard. He quotes with approval (73) Michael Dummett's suggestion that the central contrast between the picture of language and meaning proposed in the *Tractatus* and that of the *Investigations* resides in a shift from a conception of statement-meaning as truth-conditional to the view that the meaning of each statement is fixed by its association with conditions of justified assertion. But Dummett, at least as I read him, never intended that reorientation to involve a total rejection of the category of fact-stating discourse. It could not be so intended with any plausibility, since, as we have noted, the historical Wittgenstein thought that we are apt to be misled by the form of our discourse in certain selected areas into thinking that its role has to be that of stating facts. He could hardly have considered that we

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were likely to be so *misled* unless he thought that form of discourse to be very often associated with the activity of fact-stating. In any case, whatever intention Dummett, or Wittgenstein, may have had, it is doubtful that it is coherent to suppose that projectivist views could be appropriate quite globally. For, however exactly the distinction be drawn between fact-stating and non-fact-stating discourse, the projectivist will presumably want it to come by way of a *discovery* that certain statements fail to quality for the former class; a statement of the conclusion of the skeptical argument, for instance, is not *itself* to be projective. But can Kripke's exposition make space for this admission? According to Kripke, what is distinctive of fact-stating is the possession by one's statements of "real truth conditions" (whatever that may mean). And how can the judgment, "*S* has (real) truth conditions," be genuinely factual if—in accordance with the platitude and the considerations of a moment ago—"*S* is true" is not?

Another way of seeing that the situation cannot really be satisfactory is to inquire what status, once the skeptical argument is accepted, is supposed to be possessed by the sort of account adumbrated by Kripke of the assertion conditions of statements about meaning and understanding. Could it yesterday have been *true* of a single individual that he associated with the sentence "Jones means addition by '+'" the sort of assertion conditions Kripke sketches? Well, if so, that truth did not consist in any aspect of his finite use of that sentence or of its constituents; and, just as before, it would seem that his previous thoughts about that sentence and its use will suffice to constrain to within uniqueness the proper interpretation of the assertion conditions he associated with it only if he is granted correct recall of the content of those thoughts—exactly what the skeptical argument does not grant. But would not any truths concerning the assertion conditions previously associated by somebody with a particular sentence have to be constituted by aspects of his erstwhile behavior and mental life? So the case appears no weaker than in the skeptical argument proper for the conclusion that there *are* no such truths; whence, following the same routine, it speedily follows that there are no truths about the assertion conditions that any of us presently associates with a particular sentence, nor, *a fortiori*, any truths about a communal association. It follows that the premises, requisite for Kripke's version of the PLA, about the community-oriented character of the assertion conditions of statements concerning meaning and understanding are not genuinely factual, and the same must presumably be said of the conclusion, that the concepts of meaning and understanding have no proper application to a private linguist.

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The skeptical solution seems to me, therefore, to be a failure. More: to sustain the skeptical argument is to uncage a tiger whose depredations there is then no hope of containing.

III. RESISTING THE SKEPTICAL ARGUMENT

Kripke himself considers two possible sources of error in the argument. The first is the assumption that facts about my former understanding of *E* must be constituted by aspects of my former behavior and mental life. Is not a more plausible candidate, a certain former *disposition,* the disposition to use *E* in certain sorts of way? Against this suggestion Kripke brings (26–37) two prima facie very telling sets of considerations to bear. First, the relevant sorts of disposition are, with respect to any particular expression, presumably finite, since all my capacities are finite; whereas, intuitively, we want the meaning of *E* to contribute toward the determination of its correct use in literally no end of potential cases. Second, meanings are, whereas dispositions are not, *normative*: I may, in certain circumstances, be disposed to use an expression in a way which is out of accord with my understanding of it and which, therefore, constitutes wrongful use of that expression; whereas I can scarcely be said to have a disposition to use an expression in a way out of accord with the way in which I am disposed to use it. Now there is, no doubt, scope for discussion about how decisive these two rejoinders are.[3] In particular, it need not be contradictory to suppose that someone may be disposed to act in a way in which he is not disposed to act—provided his dispositions are appropriately *stratified.* So much, at any rate, is certainly part of our ordinary concept of a disposition: almost all the dispositional properties about which we ordinarily speak are such that their display is conditional on the absence of certain interfering factors, and there is no contradiction in the idea that such interference might be widespread and even usual. The matter is obviously one of some subtlety. Here I can do little more than record my own view that Kripke is ultimately right, at least as far as our intuitive conceptions of meaning and understanding are concerned. Understanding an expression is, intuitively, more like an ability than a disposition.[4] Roughly, it is the (fallible) ability to suit one's employment of the expression to certain constraints. Even at the most fundamental level, then, and when nothing interferes with the exercise of a disposition, there ought to be a distinction between what somebody's understanding

[3] See, e.g., Simon Blackburn, "The Individual Strikes Back", *Synthese,* LVIII, 3 (March 1984): 281–301, and Graeme Forbes, "Skepticism and Semantic Knowledge", *Proceedings of the Aristotelian Society,* LXXXIV (1983/4): 221–237.

[4] Cf. G. P. Baker and P. M. S. Hacker, *Wittgenstein: Meaning and Understanding,* vol. I of *Essays on the Philosophical Investigations* (Oxford: Blackwell, 1983), ch. XVI.

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of *E* requires of him and the use of that expression which he actually makes; it is just that, if nothing interferes with the exercise of the disposition, the use he makes *will be* the use required of him. You could put the point by saying that, intuitively, understanding generates *rule-governed* behavior; to suppose that it is at some fundamental level simply a matter of a disposition is to ignore the distinction between suiting one's behavior to a rule and merely behaving in such a way that, when the rule is construed as a descriptive hypothesis, it fits what one does.

A second response to the skeptical argument which Kripke discusses (41–50) is the idea that meaning green by 'green' "denotes an irreducible experience, with its own special *quale*, known directly to each of us by introspection." If there were such an experience "as unique and irreducible as that of seeing yellow or feeling a headache," then—in the presence of the relevant idealizations—it could simply be recalled in response to the skeptic's challenge and that would be that. Kripke's response to this proposal, drawing extensively on themes explicit in the *Investigations*, is surely decisive. Quite apart from the introspective implausibility of the suggestion, it is impossible to see how such an experience could have the *content* that understanding is conceived as having, could have, as it were, something to say about the correct use of *E* in indefinitely many situations. There might, indeed, be a distinctive experience *associated* with meaning so-and-so by *E*; but then, in order for recall of the experience to meet the skeptic's challenge, it would be necessary additionally to recall the association—and that would presuppose recall of one's former understanding of *E*, the possibility of which is exactly what is at issue.

There are, however, a number of other ways in which the skeptic's routine, seductive though Kripke's presentation makes it seem, is open to serious question. One concerns the play the skeptic makes with the *finitude* of previous linguistic behavior. There is no question, of course, but that the relevant behavior is finite and that it is thereby debarred from supplying a *conclusive* ground for affirming that your former understanding of *E* was, indeed, so-and-so. But to suppose that it follows that there is no rational basis for preference among indefinitely many competing hypotheses, all of which are consistent with your previous linguistic behavior, is tantamount to the supposition that Goodman's "new riddle of induction" admits of no solution. This point does not, of course, depend on the fact that we actually used a Goodman-type example in the development of the skeptical argument. Rather, Goodman's riddle is exactly the challenge to explain in what, if any, sense it is ra-

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tional to prefer, on the basis of finite evidence, the sorts of general hypotheses which we invariably do prefer to any of the other indefinitely many alternatives whose formulation he illustrates. Of course, this assimilation is not in itself a satisfactory rejoinder to Kripke's skeptic. But it does at least show him up for a fairly familiar animal. And it teaches us that we ought not to regard the skeptical argument as, so to speak, establishing a theorem unless we think it right to despair of a solution to Goodman's riddle.[5]

What *is* unsatisfactory about the suggestion is that it gets the intuitive epistemology of understanding wrong. Recognition that a certain use of an expression fits one's former (and current) understanding of it would not, it seems, except in the most extraordinary circumstances, have to proceed by inference to the best semantic explanation of one's previous uses of that expression. The kind of fact—if, against the skeptical argument, there can indeed be such a fact—which having formerly had a particular understanding of an expression is, is misrepresented by this response.

There is, however, a further response, focusing on the second stage of the skeptical argument, at the point where it is argued that, no matter how rich a battery of explicit thoughts you may

[5] Afficionados of Kripke's text might feel that he, in effect, answers this point (38). "Let no one—under the influence of too much philosophy of science—suggest that the hypothesis that I meant plus is to be preferred as the *simplest* hypothesis. I will not here argue that simplicity is relative or that it is hard to define, or that a Martian might find the quus function simpler than the plus function. Such replies may have considerable merit, but the real trouble with the appeal to simplicity is more basic. Such an appeal must be based on a misunderstanding of the skeptical problem, or of the role of simplicity considerations, or both. Recall of the skeptical problem was not merely epistemic. The skeptic argues that there is no *fact* [my italics] as to what I meant, whether plus or quus. Now simplicity considerations can help us decide between competing hypotheses, but they obviously can never tell us what the competing hyoptheses are. If we do not understand what two hypotheses *state*, what does it mean to say that one is 'more probable' because it is 'simpler'? If the two competing hypotheses are not genuine hypotheses, not assertions of genuine matters of fact, no 'simplicity' considerations will make them so."

I do not wish to suggest that canons of simplicity provide an appropriate response to Goodman's riddle. However, Kripke's point is general. Whatever criterion of preferability among competing hypotheses we come up with, its application can be appropriate only if we do genuinely have competing *hypotheses*, only if there is some "fact of the matter" about which we are trying to arrive at a rational view. Therefore—or so Kripke's thought presumably runs—we beg the question against the skeptic in appealing to any such criteria at this stage. But this surely gets everything back to front. It is only *after* the skeptical argument has come to its conclusion that the skeptic is entitled to the supposition that there is indeed no such fact of the matter. In the course of the argument, *he* cannot assume as much without begging the question. At the stage at which we might appeal to the sort of refined methodology which could be used to answer Goodman's riddle, there simply is not yet any basis for thinking that talk of meaning and understanding is not factual. For what is, I think, a better response to the point, see the sequel in the text.

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formerly have entertained concerning your understanding of *E*, these thoughts will not turn the trick. Kripke's skeptic discounted the attempt to bring your previous general thoughts against unwelcome interpretations of your previous use of *E*, on the grounds that you thereby presuppose knowledge of the proper interpretation of those thoughts—which is, in detail, knowledge of the very putative species currently under suspicion. This can seem reasonable. On inspection, however, it cannot *always* be possible to justify a presumed genre of knowledge "from without" in the way the skeptic is here demanding. At any rate, it is obvious enough that, if we were to allow the propriety quite generally of this skeptical move, the results would be calamitous. Imagine, for example, a skeptic who questions a claim about my former perceptions, say, "Yesterday, I saw it raining." And suppose the ground rules are as for the dialogue with Kripke's skeptic; that is, I am to be permitted to adduce any relevant fact so long as I do not thereby presuppose that there is such a thing as knowledge of what I formerly perceived—since it is of belief in the very existence of the genre of knowledge that the skeptic is demanding justification. So I cannot simply claim to remember what I perceived: my ammunition will be restricted to my present *seeming-memories*, the presently available testimony of others, presently accessible putative traces, like damp ground, etc. and meteorological office and newspaper records. It ought to be a straightforward, if tedious, exercise for the skeptic to accommodate all that without granting me the truth of my claim about my perception of yesterday's weather. So I can know "all relevant facts" without knowing anything about what I formerly perceived. So there is no fact of the matter about what I formerly perceived. So, since the arguments will work just as well in the future when now is "then," there is no fact of the matter about what I *presently* perceive. So, since the argument applies to all of us, there is no such thing as perceptual knowledge. "There's glory for you!"

The trouble, evidently, is the assumption that knowledge of a former perception has to be *inferential*, that the ultimate grounds for such knowledge must reside in knowledge of a different sort. That is true only if knowledge of what I am presently perceiving is inferential; otherwise, the skeptic may satisfactorily be answered simply by recalling what one formerly perceived. So, too, Kripke's skeptic persuades his victim to search for recalled facts from which the character of his former understanding of *E* may be *derived*. And that is fair play only if knowledge of a *present* meaning has to be inferential; otherwise the skeptic is satisfactorily answered simply by recalling what one formerly meant.

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The claim, then, is that the methodology of the skeptical argument is appropriate, if ever, only to cases where it is right to view the putative species of knowledge in question as essentially inferential. And no ground for that supposition in the present case has so far been produced. But if it is to be possible simply to recall the character of former meanings, can the requisite presupposition, that knowledge of present meanings may be noninferential, really be made good? Kripke, in effect, confronts this suggestion when he considers the possibility (50/1) that meaning so-and-so by *E* might simply be an irreducible, *sui generis* state, a state "not to be assimilated to sensations or headaches or any 'qualitative' states, nor to be assimilated to dispositions, but a state of a unique kind of its own." His reply, only very briefly developed (52/3), is that it is utterly mysterious how such a state could have the requisite properties, in particular how, although a finite state realized in a finite mind, it could nevertheless have the potential infinity of content that the normativity of meaning requires. How can there be a state which each of us knows about, in his own case at least, noninferentially and yet which is infinitely fecund, possessing specific directive content for no end of distinct situations?

This may be a good question. But Kripke's discussion contrives to leave the impression that it is rhetorical, that we have not the slightest idea what such a state might be. Whereas a little reflection shows that both these features—noninferentiality and indefinite "fecundity"—are simply characteristic of our standard intuitive notion of *intention*. Normally, we are credited with a special authority for the character of our own intentions; asked about them, it is considered that we ought to know the answer, and, saving lying and slips of the tongue, etc., that our answers should be given a special weight. Admittedly, this authority does not have to be taken to suggest noninferential knowledge; it might be, for example, that it derived from authority for the premises of an inference—say, certain occurrent thoughts. But to think of self-knowledge of intention, in any case where the subject would be credited with authority, as invariably based on inference from associated occurrent thoughts is to caricature the ordinary notion. For one thing, each of us regularly carries out intentional acts without necessarily thinking about what we are doing at all. Usually these are routine activities in which we are expert. It is perfectly proper to say of such activities that they are knowingly and intentionally performed and, indeed, that they are preceded by the appropriate intention. (If you were asked, in advance, whether you had the appropriate intention, you would unhesitatingly confirm that you did.) Notice also that we can in general make no ready sense of the question,

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"How do you know?" directed at an avowal of intention; if there were an inference in the offing, the question ought to admit of a straightforward answer. But the decisive consideration is this. Even when an intention is accompanied by certain occurrent thoughts relating to its content or the circumstances of the (envisaged) course of action, one's knowledge of the character of the intention is not to be thought of as achieved via reflection on the content of those thoughts. If it were, by what principle could I assure myself that *those* were the thoughts on which I should be concentrating, rather than some other recent (or, if I am clever enough, simultaneous) train? To come to know that you have a certain intention is not to have it dawn on you that you have an intention of *some* sort and then to recover an account of what the intention is by reflecting upon recent or accompanying thoughts. It is the other way round: you recognize thoughts as specifying the content of an intention that you have *because* you know what the intention is an intention to do.

What of the mysterious fecundity? Well, suppose I intend, for example, to prosecute at the earliest possible date anyone who trespasses on my land. Then there can indeed be no end of distinct responses, in distinct situations, which I must make if I remember this intention, continue to wish to fulfill it, and correctly apprehend the prevailing circumstances. But *if* we are at ease with the idea that my intention has a general content, noninferentially known to me, then there is no more a puzzle about the "infinity" of this content than there is a puzzle about the capacity of any universally quantified conditional, $(x)(Fx \rightarrow Gx)$, to yield indefinitely many consequences of the form, $Ga, Gb, \ldots$, when conjoined with corresponding premises of the form, $Fa, Fb, \ldots$.

I want to stress that this is merely to describe what seem to be features of our intuitive notion of intention. The notion is not unproblematic. It could be that it is radically incoherent. The fact remains that it is available to confront Kripke's skeptic, and that, so far as I can see, the skeptical argument is powerless against it. The ordinary notion of intention has it that it is a characteristic of mind—alongside thought, mood, desire, and sensation—that a subject has, in general, authoritative and noninferential access to the content of his own intentions, and that this content may be open-ended and general, may relate to all situations of a certain kind. In order, then, to rebut the skeptical argument, it would have sufficed, at the point where the skeptic challenged you to adduce some recalled mental fact in order to discount the grue-interpretations, to recall precisely your former intention with respect to the

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use of 'green'. To be sure, any *specification* that you might give of the content of that intention would be open to unwelcome interpretation. But, if you are granted the intuitive notion of intention, you can reply that you do not in any case know of the content of an intention via a specification of it; rather, to repeat, you recognize the adequacy of the specification because you know of the content of the intention.

The point, in summary, is not that it is particularly *comfortable* to think of your former meaning of 'green' as consisting in your having had a certain general intention, construed along the lines of the intuitive conception, but rather that the skeptical argument has absolutely no destructive force against that proposal.

IV. KRIPKE'S WITTGENSTEIN

I conclude with but the briefest indication of the most important difference, as I see it, between Kripke's Wittgenstein and Wittgenstein.

There is an evident concern in the *Investigations* with a large class of psychological predicates which, like intention intuitively conceived, *seem* to have a content that can somehow transcend that of any accompanying thoughts in the subject's mind. Examples are: recalling how a piece of music goes (without hearing it right through "in one's head"); deciding to have a game of bridge (without thinking through all the rules); realizing how to continue a series (without *per impossible,* thinking through the entire infinite expansion); grasping the meaning of an expression "in a flash" (without having all its possible uses run before one's mind); and so on. Each of these predicates, it seems, can come to be true of a subject quite abruptly, yet involves some sort of reference to things he need not, on that occasion, think about explicitly. Wittgenstein thought that we were greatly prone to misunderstand the "grammar" of these notions and to form quite false pictures of the nature of the connection that obtains between the psychological state of someone of whom such a predicate comes to be true and the "absent aspects" noted in the parentheses above. In particular, there need be *no* connection between the subjective content, properly so regarded, of such states and the detail of the "absent aspects".[6] Accordingly, the *normative* power of intention—the determinacy, when it is determinate, in the matter of whether a particular course of conduct fulfills a prior intention—cannot always be accounted for by reference only to the previous subjective content of the subject's psychological states. Wittgenstein's conclusion, however, is

[6] For an excellent discussion of these examples, see Malcolm Budd, "Wittgenstein on Meaning, Interpretation and Rules," *Synthese,* LVIII, 3 (March 1984): 303–323.

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emphatically *not* that there is no such thing as the fulfillment of a prior intention—the conclusion, in effect, of Kripke's skeptic.

> Is it correct for someone to say: "When I gave you this rule, I meant you to . . . in this case"? Even if he did not think of this case at all as he gave the rule? *Of course it is correct.* For "to mean it" did not mean: to think of it (*Investigations* §692; my italics).

Rather, a satisfactory philosophy of intention has to validate our claim to noninferential authority for our present (and previous) intentions without succumbing to the mythology of infinite, explicit introspectible content. The intuitive conception of intention utilized against Kripke's skeptic above perennially tempts us toward this mythology. But there has to be something right about it if—*pace* those who would wish to reanimate a dispositional account of meaning and intention—Kripke's skeptic is not to win the day.

It is this dilemma which is prominent in the last sections of part I of the *Investigations* (§ 591 to the conclusion), sections about whose evaluation there is so far little consensus. The insight that there *is* a problem here, of the most profound importance for the philosophies both of language and of mind—whether or not Wittgenstein solved it—is one of the principal lessons of the *Investigations,* and one which Kripke's book ought to make it easier to learn.

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BOOK REVIEWS

Frege's Conception of Numbers as Objects. CRISPIN WRIGHT. The Scots Philosophical Monograph Series. Aberdeen: Aberdeen University Press, 1983. xxi, 193 p. £8.50.

Despite its title, this book is no scholarly study of Frege on the numbers. Rather it is an original contribution to the philosophy of mathematics, an attempt to develop and defend the conception of numbers expressed in Frege's *Foundations of Arithmetic.* Wright takes this to consist of three independent elements: (1) number-theoretic realism—statements about the numbers are determinately true or false in virtue of states of the world irrespective of our ability to ascertain them; (2) number-theoretic platonism—the numbers "constitute a unique domain of genuine objects" (xx); (3) number-theoretic logicism—logic is the epistemological foundation for

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number theory.[1] Wright's intricate argument, which is delightfully rich and full of side lines and blind alleys, often fails clearly to separate the defenses of these three Fregean themęs. As I read him, Wright devotes the bulk of the book to defending a version of number-theoretic platonism, turning only in the last of the four chapters to logicism and nowhere to an explicit defense of realism.

Wright follows Michael Dummett both in interpreting Frege's context principle and in taking it to be the central premise of a positive philosophy of arithmetic.[2] Although Wright develops his interpretation of the context principle over several sections of the earliest chapter, the following late passage serves to illustrate both it and its chief application:

> . . . if a sortal concept of number is available, and normal criteria determine that it has instances—that is, contexts of relevant types are true which contain terms purporting to denote natural numbers—then there *are* such things (129).

The idea here is that, once we have shown that number terms function as singular terms in an appropriate body of truths, that is all that we must do to show that numbers are objects and exist. There is an uncontroversial weak reading of this thesis—one that would lead us to see recent debates in the philosophy of mathematics as concerned with whether number words are genuine singular terms and whether certain statements containing them are literally true. Given this construal, I have little objection to Wright's concession that he has nothing to offer in support of his principle except that "great difficulties will beset any attempt to describe a coherent alternative (52)." On the other hand, Wright wants to get more purchase from his principle than the weak reading can furnish. He intends to use it, on the one hand, to reject nominalist attempts to dispense with the language of number theory or literal number-theoretic truth and, on the other hand, to argue that the our ordinary use of number words as singular terms and the truth of number-theoretic statements "by ordinary criteria" are sufficient to prove that there are numbers.[3] Since this threatens to promote the wildest sort of ontological splurges, Wright has built in some safeguards. Although "He did it for the sake of harmony" may well be

[1] Throughout this review I will follow Wright and use 'number' to refer to the finite cardinal numbers.

[2] There are other interpretations of Frege's principle and the role it plays in *Foundations*, but, in keeping with the thrust of Wright's book, I will set aside questions of Frege exegesis.

[3] This and other points are discussed in greater depth in Hartry Field's critical study of Wright's book, forthcoming in *The Canadian Journal of Philosophy*.

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a true sentence by ordinary criteria, Wright's principle opens no door for such pseudo-entities as sakes; for *sake* is not a genuine sortal concept: there are no identity conditions for sakes; we cannot count them, nor can we distinguish sakes from other kinds of objects.

The problem now becomes one of showing that the lines have not been drawn too firmly, that number words do function as genuine singular terms, and that *number* is a genuine sortal concept. Wright adapts Frege's reasoning to argue that expressions of the form 'the number of *F*s' (symbolically, "N*xFx*") do function as singular terms, and invokes Frege's principle:

(N=) N*xFx* = N*xGx* if and only if there is a 1-1 correlation between *F* and *G*.

to show that there are identity conditions for numbers. But for *number* to be a full sortal concept, it must be determined for every object whether or not it is a number. Frege rejects (N=) as a definition of number because it fails to determine whether Julius Caesar is a number. Wright in turn rejects Frege's ultimate definition in terms of extensions of concepts for pushing the "Caesar problem" back to extensions, and proposes to solve the Caesar problem at the level of numbers. His insight is that numbers cannot be persons since numbers and persons have radically different identity conditions. Generalizing this leads him to the principle:

(N^d) *Gx* is a sortal concept under which numbers fall only if there are, or could be, singular terms "*a*" and "*b*" purporting to denote instances of *Gx* such that the truth-conditions of "*a* = *b*" could adequately be explained as those of some statement to the effect that a 1-1 correlation obtains between a pair of concepts (117).

This does not dispose of the Caesar problem entirely, however; for numbers might be classes, depending upon how strongly one construed the notion of adequate explanation used in (N^d)—an issue which Wright leaves open. Indeed, if (N^d) does not distinguish numbers from classes, it fails to distinguish them from any of the infinitely many progressions of equivalence classes with respect to some equivalence relation defined in terms of 1-1 correlation. This does not daunt Wright, however, since he thinks that would simply show that 'class' and 'number' are mutually vague—as are, for instance, 'person' and 'body'. More importantly, he sees the phenomenon involving the numbers as just a special case of a referential relativity which extends to all other categories of objects. Thus one may appeal to it to deny reference to number words only

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on pain of denying it to other terms as well.[4] Wright rests his defense of *number* as a sortal concept at this point.

In the final chapter Wright argues for a restricted form of logicism. The idea is to take (N=) as the sole nonlogical axiom and derive the Peano axioms from it using second-order logic. The formal derivation, which mimics Frege's, appears to come off without a hitch. But why count this as logicism? Because (N=) can serve as an *explanation* of '$\mathrm{N}xFx$' in logical terms. (Wright does not call it a "definition" because it will not permit us to eliminate all occurrences of 'N'—e.g., its occurrences in '$(\exists y)(y = \mathrm{N}x(x = x))$' or '$\mathrm{N}x[x = \mathrm{N}y(y = y)] = \mathrm{N}x(x = x)$'.) Yet he believes that someone who understood logic and was given (N=) could come to grasp the concepts of number theory through a series of stages. At the first stage, he would be able to understand sentences of the form '$\mathrm{N}xFx = \mathrm{N}yGy$', with no nested occurrences of 'N', as having the same truth conditions as 'F and G are in 1-1 correlation', and he would fully understand the latter sentence on the basis of his logical knowledge. He could also determine the falsity of sentences of the form '$q = \mathrm{N}xFx$', where 'q' is a singular term that he already understands, by applying (N^{d}) (which Wright assumes that the "trainee" could arrive at on his own). Having got this far, Wright is willing to credit the trainee with an understanding of predicates of the form '$\ldots = \mathrm{N}xFx$' in which no nesting of 'N' occurs. But that suffices for him to understand sentences stating that such predicates are in 1-1 correlation, and thus the process can be iterated to

[4] I think that the connection between the alternative reductions for the numbers and general referential relativity is more complicated than Wright thinks it is. First, one could rightly complain that, even if the references of all concrete terms are held fixed, the references of abstract terms remains infinitely indeterminate. Second, the referential relativity of concrete terms is demonstrated by showing that we can permute any model of the language of concrete objects while preserving truth values, whereas Wright is concerned with alternative ontological reductions. On the other hand, Wright is surely right in pointing out that the threat of alternative ontological reductions cannot be restricted to the numbers or even to other abstract entities. Alternative reductions for functions, real numbers, ordered pairs, and other mathematical concepts are well known. More generally, let us suppose that a sortal concept is specified by a set of conditions which includes identity conditions of the form:

$$a = b \text{ if and only if } \ldots a \ldots b \ldots .$$

Then, given one model of the total set of conditions, there is another model of the same conditions obtained by permuting some of the objects of the original model. The identity conditions now take the form:

$$p(a) = p(b) \text{ if and only if } \ldots . p(a) \ldots p(b) \ldots .$$

but, since $p(a) = p(b)$ if and only if $a = b$, they serve as identity conditions for the original set of objects too. Given a candidate ontological reduction of the objects of a theory T to those of a theory T', it will often be possible to define a permutation such as the above within T', and then the existence of alternative ontological reductions will be demonstrable within T'.

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bring him to grasp sentences in which a single nesting of 'N' occurs, and so on, until he learns all the concepts of number theory.

There are serious difficulties with this procedure. First, the Caesar problem threatens to arise again in the training context. Whether classes are among the objects with which the trainee is already familiar or among those embraced by him after he has mastered number theory, it is still undetermined whether (N^d) settles identities between numerical terms and class terms. (Similar problems will arise with respect to ordinal numbers, since two finite ordinals are identical just in case the fields of any well-orderings they measure stand in a 1-1 correlation.)[5] But if (N^d) does not resolve these difficulties, I do not think we can fall back on Wright's suggestion that we should expect a certain amount of mutual vagueness between *number* and certain other sortal concepts. That suggestion is appropriate for our own case, where, having already mastered the concept of number, we find no fact to the matter of whether numbers are classes. But how could Wright's trainee have our perspective? We are troubled only by the borderline cases, but at any stage the trainee has mastered only a fraction of the *clear* cases. Thus I do not see how he could regard his inability to fix truth conditions for a sentence of the form '$q = \mathrm{N}xFx$' as anything short of disastrous.

Aside from this, Wright's attempt to show that all numerical concepts can be explained in terms of logical concepts via (N=) runs into a difficulty similar to one which vitiated Frege's attempt (in *Grundgesetze,* sec. 31) to show that the metalinguistic version of his axiom V sufficed to assign references to all terms containing his abstraction operator. Consider the definition of cardinal number:

$$\mathrm{CN}y \leftrightarrow (\exists F)(y = \mathrm{N}xFx)$$

In order to understand this, our trainee must make sense of the expression '$\mathrm{N}xFx$' where 'F' is a concept variable. It is simply unclear that the training process will enable him to master this use of 'N'.[6] Wright, who discusses a related objection in a long footnote (180-184), believes that the trainee could come to understand a sufficient number of specific terms of the form '$\mathrm{N}xFx$' to master the use of N-terms with concept variables. But I am not convinced. Consider the sentence '$(\exists y)(\exists F)(y = \mathrm{N}xFx)$'. If our trainee is quite

[5] Of course, classes are unnecessary for Wright's foundation for analysis, but surely ordinals are not. Besides, he himself dismisses dispensability arguments (129).

[6] A similar problem arises with respect to the definition of 'successor'. I discuss the many difficulties with *Grundgesetze* sec. 31 in my "Frege's Proof of Referentiality," forthcoming in a Synthese Library volume edited by Leila Taiminen and Jaakko Hintikka.

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unsophisticated, he may have simply generalized a rewrite rule from (N=) and his training process. If so, he will not know how to start processing the sentence. On the other hand, if he is knows enough logic to know about the distinction between referential and substitutional quantification and real and virtual numbers—to borrow Quine's terms for classes—then he may conclude that he has no grounds at all for assigning truth conditions to the sentence.

Limitations of space have prevented me from discussing many of the very interesting things Wright says (e.g., about reference to numbers and other abstract objects). Wright's style of writing and argumentation is too convoluted for my tastes. This, coupled with a lack of constancy in his position, made the book hard going for me, and, accordingly, I do not think it fulfills Wright's hope that it will help undergraduate students (ix). Yet I learned much from this book and recommend it to all serious students of the philosophy of mathematics and related areas of epistemology and metaphysics.[7]

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Mathematics in Philosophy: Selected Essays. CHARLES PARSONS. Ithaca, N.Y.: Cornell University Press, 1984. 365 p. $42.50.

Nine of the eleven essays in this volume appeared in the years 1964–1977. It is a service to have brought them conveniently together, for several of them were major contributions and are frequently cited. The nine are reprinted here without emendation, but notes and postscripts have been added, and there is a twenty-page general introduction.

One of the hitherto unpublished essays is new; the other, "Quine on the Philosophy of Mathematics," was written in 1975 for a Schilpp volume that had been projected at that time. Because of the attention paid me in that essay and others, I can perhaps be excused for devoting part of this review to my ideas.

The first essay is "Ontology and Mathematics." What are numbers? Are they to be identified with sets in von Neumann's way, or Zermelo's, or Frege's? All these ways and others deliver the required structure, but they are incompatible. Such considerations

[7] I would like to thank Crispin Wright for his comments on an earlier draft of this review.

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have given rise to the

> . . . commonplace of the working philosophy of modern mathematicians that they are concerned with *structures* . . . such that it does not matter what the underlying objects are or . . . it can matter only if the structure is part of a larger structure (41).

Parsons subsumes this mathematical structuralism under my more sweeping thesis of ontological relativity, to the effect that *all* reference to objects, concrete as well as mathematical ones, can be reinterpreted in any arbitrary one-to-one fashion without falsifying any of our sentences or disturbing any of the evidence that supports them. He represents my thesis as derived from indeterminacy of translation, and historically he is right; but my more direct and conclusive argument, subsequently used,[1] is by appeal rather to what I call proxy functions.

Parsons sympathizes with one's natural reluctance to ascribe reality to abstract objects such as numbers. He proceeds to show that to interpret elementary number theory we need talk only of concrete inscriptions and how we can go on making them. He recognizes that the modality of 'can' here obstructs direct application of my criterion of ontological commitment, preventing me from conceding that the construction is nominalistic. But that is the least of it; I prefer numbers and other abstract objects to modal operators anyway. Leveling of modalities is in my view a major service of abstract objects.

Taking another logical tack, Parsons contemplates elementary number theory under substitutional interpretation of its quantifiers. He rightly reports my attitude toward the ontology of substitutional quantification; namely, that we can assess it only by paraphrase into an objectually quantified theory, and that the ontology is then apt to consist of expressions, or, by Gödel numbering, the natural numbers after all. Where the system under consideration is elementary number theory, then, the ontological upshot is the same whether we take the quantifiers as substitutional or objectual.

Next he queries the ontology of a more meager arithmetic, in which the variables are unquantified—hence, in effect, schematic letters for numerals. If so construed, the formulas are schemata rather than sentences; so any question of ontology would devolve rather upon the actual sentences that the schemata schematize. But those sentences are mechanically decidable and hence, I should say, ontologically empty; they are all definable as transcriptions of '$0 = 0$' and '$0 \neq 0$'. One's actual activity with such a theory, how-

[1] *Theories and Things* (Cambridge, Mass.: Harvard, 1981), Essay 1.

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ever, would go forward mainly in a metatheory, and there an ontology at least of expressions, or in effect again natural numbers, would no doubt be called for.

In another venture in ontological economy Parsons begins with the natural numbers as real and then generates sets of them, sets of such sets, and so on, in such a way as to identify each predicative set with a natural number.

His second essay is "A Plea for Substitutional Quantification," where he proposes quantifying substitutionally over abstract objects and objectually over concrete ones. The plan is congenial to one's feeling that abstract objects are "not 'real' independently of the expressions for them" (66). It invites a standard of ontological commitment alternative to the one associated with objectual quantification. It invites a double standard, one for particulars and one for universals. This conforms nicely to the old duality of existence and subsistence, endowing it at last with technical significance.

His is indeed a philosophically appealing way of viewing quantification over classes, as long as they are predicative. In impredicative cases, however, the familiar truth conditions for substitutional quantification can fail to wind down. The substituted class abstract can contain a class quantifier in turn, whose subsequent elimination traps us in a cycle. We may have to resort to a Tarskian truth definition, and in that event it is not clear that our quantification was justly described as substitutional. I gather from correspondence that Parsons agrees here. He has espoused his substitutional treatment only for predicative classes.

A technical problem is raised by collisions between the objectual quantification over individuals and the substitutional quantification over classes. Parsons resolves it by skillfully refining the truth conditions governing the substitutional quantification of sentences containing free objectual variables.

On another point his misgivings strike me as over-scrupulous. He notes that the semantics of substitutional quantification calls for quantifying over expressions in the metalanguage, and that these, being types rather than tokens, are abstract objects in turn. If this quantification is objectual, he reasons, then the substitutional interpretation of quantification over abstract objects has bogged down after all; and if it is taken as substitutional in turn, then our semantics of substitutional quantification faces an infinite regress. For my part, on the other hand, I see no distinction between substitutional and objectual quantification when all the objects are blessed with designators; and the expression types, like the natural numbers, are so blessed.

The third essay explores the relation between mathematical

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common sense and its axiomatization. Gödel's incompleteness theorem is set over against the intuitive truth of his undecidable sentence. Gödel's theorem about consistency proofs is set over against Gentzen's use of transfinite induction to prove the consistency of number theory. A hierarchy of ever stronger systems is generated by systematically incorporating erstwhile indemonstrable truths. A similar hierarchy is generated in set theory by systematically incorporating further truth predicates, each widening the range of expressible sets.

The reading public is not clearly envisioned here. Readers who are more than abreast of much of the technicality will be caught short at other points: what is Cantor normal form? what are those little superscripts for? (83, 87)

Some things could be done more simply. Tarski's theorem, to the effect that truth is indefinable in a consistent object language, is said to be "almost trivially" demonstrable, and the proof is then rather obscurely sketched (76). If the theorem were put in terms rather of satisfaction, it could really be proved trivially and clearly: the open sentence 'x does not satisfy x' directly encapsulates Grelling's paradox. On another page we encounter, as so often in model theory, the ponderous phrase 'functions from D^n to [truth values]' (76) where a straightforward 'subsets of D^n' would tell the whole tale.

The next two essays are given over to meticulous Kantian scholarship. Kant held that our knowledge of the external world is limited to objects of possible experience, but still he accepted the infinite divisibility of space. Parsons refines the pertinent notions, notably that of possibility, with a view to easing the conflict, but concludes that Kant's position is not fully defensible.

Turning to Kant's doctrine that arithmetic is synthetic a priori, Parsons examines and rejects a suggestion of Beth and Hintikka that would trace the syntheticity to what I have called the flagged variable of universal generalization and existential instantiation. Trying a different line, Parsons expands '$2 + 2 = 4$' into first-order logic by Frege's method of numerically definite quantification and then offers two conjectures as to why it might be viewed as synthetic: (1) the expansion is a conditional whose proof requires more quantifiers than appear in its antecedent, and (2) existence assumptions are needed to preclude truth also of '$2 + 2 = n$' for higher n. Finally he speculates inconclusively on Kant's view of the place of intuition in arithmetic.

The next essay is squarely on Frege. Parsons muses that

> Frege must have thought that his choice [of how to model numbers in set theory] was more natural than any alternative (155).

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and he surprises me by wondering why. A number is the size of a class; hence it is the attribute of being a class just that large; and hence, extensionally conceived, it is the class of all classes just that large. Parsons observes that a number so conceived would itself be too large a set for today's usual set theories; however, Frege was innocent as yet of the antinomies and the limitations of size that they prompted. Cantor, I grant, was heedful of limitations of size, but he was aware also, precociously, of antinomies.

Parsons rightly rejects Frege's claim to have reduced arithmetic to logic, pointing out that set theory is involved. The point controverts Frege's claim to have shown arithmetical truth to be analytic. Existence of sets is required, and that, Parsons holds, is scarcely analytic.

Parsons next questions whether Frege and others really reduce arithmetic even to set theory. We depend on mathematical induction, he argues, in the very process of devising an adequate set theory and verifying its adequacy to arithmetical purposes. Evidently, then, what Parsons wants in the way of reduction is not just a truth-preserving translation; he is concerned also for the phenomenology of the thought processes. This concern will be seen to dominate some later pages.

In his seventh essay, "Quine on the Philosophy of Mathematics," he suggests that

> A Quinian argument against the necessity of mathematics would [rest on a claim that] mathematics is not known a priori (178).

and then he questions the supposed argument. I feel like a third party here, owning no such claims or argument. Necessity makes sense to me only as indexicals do; namely, relative to situation and context. 'Necessarily' merely heralds a sentence that the speaker sees as following from some belief or assumption that his interlocutor shares; such is the paradigmatic use. Again Parsons has me wrong when he writes:

> Quine seems not to challenge the assertion that the axioms and theorems of mathematics have the character of laws rather than accidental generalization (183).

Law for me is on a par with necessity: context-bound. In a footnote (176n) of slightly later vintage he sets the record straight.

What then of logical truth? He represents me as recognizing it. Yes, I recognize the notion as I recognize the notions of chemical truth, physiological truth, and the rest; namely, in terms of the essential occurrence of words from the appropriate lexicon. If this is all that is meant by logical necessity, well and good. Parsons, how-

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ever, then contemplates explicating a modal necessity operator by disquotation of logical truth. He notes that modal statements so construed turn out contingent, because of the contingency of English grammar and semantics.

Parsons' motive in pursuing this and other degrees of modality is the quasi-nominalistic one noted earlier: avoidance of the unending reification of numbers in favor of the unending possibility of augmenting inscriptions. But in the end he apprehends a circularity in basing mathematics thus on a physical possibility, since "physical theory as it stands is founded on mathematics" (185/6).

Averse still to according mathematical objects full-fledged existence on a par with bodies, he tries viewing them as incomplete—thus recurring to the structuralism noted in the first essay. Some trace of substantiality is retained by relating these structures to concrete representations. By counting spatiotemporal regions as concrete, he avails himself of a fund of 2^c representations, where c is the power of the continuum. He notes that the expedient leaves higher reaches of set theory unexcused.

He recognizes that for me sets and concrete objects are ontologically on a par; 'exists' is univocal. "However," he pursues,

> the notion of object in set theory, and the structures whose possibility it postulates, are much more general than the notion of . . . physically representable structure. How can [Quine] still maintain that these possibilities are "natural" possibilities and that the necessity of logic and mathematics is not "higher"? (193)

So much of mathematics as is wanted for use in empirical science is for me on a par with the rest of science. Transfinite ramifications are on the same footing insofar as they come of a simplificatory rounding out, but anything further is on a par rather with uninterpreted systems. Possibility and necessity are as may be.

Parsons comes closer to my views when he cites my "claim that there is not a clear line between mathematics and empirical science" (193). Well, I can draw lines between all manner of disciplines, in terms again of essential occurrences of words; but I equate them epistemologically. As for the disparity of subject matter between physics and mathematics, which he stresses (195), let us remember that physics quantifies over numbers as well as over physical objects.

In concluding this essay Parsons contrasts my conception of classes, which he justly characterizes as Fregean, with the hierarchic or iterative conception that "is characteristic of contemporary thought on the foundations of set theory" (198). The tacit psycho-

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genesis of classes (or sets; no difference here) was indeed hierarchic, on my theory, but the levels of the hierarchy merged as they emerged, and impredicativity prevailed.[2] The eventual separating of levels, all the while condoning impredicativity, was an artifice for blocking the antinomies. Alternative artifices to that end are my "New Foundations" and its variant in *Mathematical Logic*. I do not agree with Parsons that

> Donald A. Martin . . . seems a little unfair in saying that NF is "the result of a purely formal trick intended to block the paradoxes" (200n).

but I hold that Martin was mistaken in not saying the same of the hierarchic conception.

Parsons notes (200n) the important difference between the range of variables in the theory of types and in "New Foundations," but is wrong in thinking that I "may have neglected to consider" this. I intended it from the start (1936), and it got into my writing in 1938.[3] In 1956 I showed further that when the theory of types is translated into a notation of general variables, a system results that is akin not to "New Foundations" but to Zermelo's.[4]

For many or most of us the terms 'set' and 'class' have been interchangeable. This lexical superfluity proved useful after 1928, however, in accommodating an innovation due to von Neumann: a new brand of classes that are not members of any classes. These have come to be known perversely as *proper* classes, and may more suggestively be called *ultimate* classes. The term 'set' has then remained limited conveniently to classes that are members of classes. In his eighth essay Parsons treats of ultimate classes.

What I have called *virtual* classes are another matter: a mere notational simulation of classes, eliminable by contextual definition.[5] Expressions for them are better seen simply as general terms, relative clauses.[6] Ultimate classes, on the other hand, like sets, figure genuinely as values of bound variables. Parsons proves that the adding of ultimate classes to a theory is equivalent to adding Tarski's two-place predicate of satisfaction. I think it a brilliant result. He notes further that this strengthening of a system is conservative; that is, it generates no further theorems couched in the original notation.

[2] *The Roots of Reference* (LaSalle, Ill.: Open Court, 1974), §§ 26–28.
[3] "On the Theory of Types," *Journal of Symbolic Logic*, III, 4 (December 1938); 125–139, esp. 132.
[4] "Unification of Universes in Set Theory," *ibid.*, XXI, 3 (September 1956): 267–279.
[5] *Set Theory and Its Logic* (Cambridge, Mass.: Harvard, 1982), §§ 2/3.
[6] See my *Methods of Logic*, 4th ed. (Cambridge, Mass.: Harvard, 1982), §§ 21, 46.

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Parsons, like von Neumann, requires his ultimate classes to be predicative; that is, the open sentences determining them must not themselves quantify over ultimate classes. In 1940 I propounded the unrestricted alternative as a supplement to the universe of "New Foundations," unsuccessfully at first,[7] and in 1942 I discussed the same unrestricted expedient in relation to Zermelo's system.[8] The Zermelo system as thus enlarged was called NQ by Wang[9] and BQ by Stegmüller,[10] and has come in later years to be attributed by Parsons and others to subsequent writings by Kelley and Morse. The results just now cited from Parsons depend, however, on the restriction to predicative classes.

Parsons' ultimate classes are all expressible as the extensions of open sentences. Accordingly, as in his second essay, he proposes quantifying substitutionally over them. This means using two styles of variables, and thus invites a distinction even between the sets and the corresponding nonultimate classes. Classes are then thought of strictly as the extensions of open sentences; some sets fail to correspond to classes, namely the unspecifiable sets, and some classes fail to correspond to sets, namely the ultimate classes. But impredicative classes change all that.

In the next essay, "The Liar Paradox," Parsons illustrates the parallelism of the semantic and set-theoretic paradoxes by coordinating propositions with classes and truth with membership. Where the predicate 'F' determines the class y, we have $x \in y$ if and only if Fx; and, correspondingly, where the sentence 'p' expresses the proposition y, we have y true if and only if p. He proposes accordingly to obstruct the semantic paradoxes by measures parallel to those taken in set theory. He lets some sentences fail to express propositions, just as some predicates fail to determine classes. Any of these missing classes, such as that of all non-self-members, can be added by enlarging the universe, as was done in the adding of ultimate classes; and the universe of propositions can be enlarged analogously. The process can be repeated indefinitely, generating in the one case a hierarchy of set theories and in the other a hierarchy of propositional theories of truth and reference. In either

[7] *Mathematical Logic* (New York: Norton, 1940; corrected edition, thanks to Hao Wang, Cambridge, Mass.: Harvard, 1951).

[8] "On Existence Conditions for Elements and Classes," *Journal of Symbolic Logic*, VII, 4 (December 1942): 157–159.

[9] Hao Wang, "On Zermelo's and von Neumann's Axioms for Set Theory," *Proceedings of the National Academy of Sciences, U.S.A.*, XXXV (1949): 150–155.

[10] "Eine Axiomatisierung der Mengenlehre, beruhend auf den Systemen von Bernays und Quine," in M. Käsbauer *et al.*, eds., *Logik und Logikkalkül* (Freiburg: Alber, 1962): 57–103.

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case the hierarchy can be seen also as a hierarchy of widening ranges of distinctively styled variables within a single inclusive theory.

But Tarski's familiar hierarchy of metalanguages can likewise be viewed as a single language with an infinite hierarchy of predicates asymptotically approaching the forbidden predicate of absolute truth. What then profited it Parsons to posit propositions?

It was just a flurry. He next tries dropping the propositions, taking truth and falsity as applying rather to sentences, and admitting truth-value gaps. It is a matter, he notes, of dropping his earlier analogy, where classes corresponded to propositions, in favor of a new analogy where classes correspond to truth values. It is a better analogy; truth values are properly akin to classes, and propositions rather to attributes. On this approach, just as not all predicates can determine classes, so not all sentences can have truth values.

He explains that a straightforward two-place predicate '*T*' of relative truth remains consistently available, where '*Twx*' means that *x* is a sentence that comes out true when the values of its bound variables are restricted to the set *w*. Ascriptions of relative truth are themselves true or false, and no gaps. Thence he derives predicates of truth and falsity that are absolute but do not cover all sentences; here come the truth-value gaps.

The essay ends with a consideration of ordinary language, inspired by Tarski's remark that the truth predicate makes ordinary language inconsistent. Parsons sees ordinary language as intensional and committed to propositions. Some sentences, then, may fail to express any propositions; here is one loophole whereby ordinary language may escape paradox. Also Parsons sees the universe of objects of quantification in ordinary language as different from occasion to occasion—another loophole. Flexibility in the interpretation of indirect discourse makes for yet further latitude. A major purpose of logical regimentation, after all, has been to sharpen and clarify the otherwise vague and ill-defined question of what follows from what. The unregimented and unregenerate vernacular is just too flabby to sustain a decisive test of consistency, as Parsons' thoughtful examination abundantly illustrates. He then ponders ways of so ruling upon the indecisive points of ordinary language as to safeguard its truth predicate from paradox.

To this ten-year-old essay he appends fifteen new pages, difficult at points, regarding subsequent work on the subject by Kripke, Feferman, Burge, Herzberger, and Gupta. The goal as he envisages it is a consistent set of rules, as neat as may be, that fit English speakers' "use of 'true' and related locutions" (266).

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In his tenth essay Parsons takes up again the hierarchic or iterative conception of sets. On this conception, typified by Russell's theory of types but not limited to it, each set is in some sense posterior to its members. But in what sense, Parsons asks? Not temporally; the picture of progressive fabrication of sets from members cannot be taken literally, despite its persistence in the literature from Cantor through Shoenfield, Wang, and others. Parsons tells how Wang elaborates the metaphor: to form a set we "look through or run through or collect together" various of the objects in a "multitude" whose "range of variability is in some sense intuitive." We must be able to "overview" the multitude, "in an idealized sense" (275). Parsons joins Wang in these phenomenological gropings, sympathetically and critically.

Unresponsive myself to the phenomenological nuances, I should have thought it sufficient to say of the hierarchic ordering that it was perhaps inspired by the metaphor of progressive fabrication and that it is intuitively somewhat sustained by it despite the aberration of impredicative sets, but that its strength lies in its perspicuity and its security from paradox. Well, almost sufficient; there is also my psychogenetic speculation (note 2 again).

The metaphor of fabrication imposes, along with the hierarchic structure, the axiom of foundation. But that axiom is warranted also, I might add, by an argument from individuation. Sameness of members serves to individuate classes only insofar as the members themselves are individuated; so an ungrounded class induces a regress.

My version of individuals as self-singletons is a violation of the axiom of foundation that can easily be accommodated, as Parsons remarks (201n), by adjusting the axiom to allow the exception. As individuals they may be supposed individuated independently of set theory as usual—e.g., by space and time. It would be regrettable to forfeit the version of individuals as self-singletons; for it confers the same gains in simplicity for which some set theorists have been prepared to abandon individuals altogether and thus divorce set theory from general science.

Let me stretch this digression to two more remarks. (1) The seeming unnaturalness of my version of individuals can be mitigated by thinking of it rather as a renunciation of singletons of individuals and a reinterpretation of epsilon as 'member of or same individual as'. (2) Though my systems in "New Foundations" and *Mathematical Logic* abound in ungrounded classes quite apart from individuals, my version of individuals as self-singletons is suited equally to Zermelo's set theory and the others.

Back, then, to Parsons' tenth essay. Later in it he recurs to

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classes, and ventures again to distinguish them from sets even when they are not ultimate. Phenomenologically, sets are gatherings up, whereas classes are extensions of predicates or generalizations of such. Resorting to modal logic, Parsons suggests that a set has its members necessarily but a class in general does not; and I am bound to say that it is a cute idea if you like necessity.

His next quandary concerns the ranges of bound variables. It disturbs him that the totality of sets over which the variables range does not itself exist as a set. He feels that the unreality of the range marks a failure in the interpretation of the variables. Note, then, that in my "New Foundations" and in *Mathematical Logic* the range of the variables is indeed accounted a set, though I had not reckoned Parsons' point among the virtues of that feature. (Nor do I have a single-minded commitment to those systems; this is clear from *Set Theory and Its Logic.*)

Parsons is exercised also about the indeterminacy of the range in respect of how far it probes the transfinite potentialities of the iterative hierarchy. A perplexing '*R*' then intrudes, with Greek subscript, and exposition falters. A cross reference to page 218 engenders hope, but 219 would have helped more, and 87n would have cleared it all up. At any rate he proposes viewing the range of variables as systematically ambiguous, in the manner of the "typical ambiguity" of *Principia Mathematica.* As an alternative he suggests a modal account in terms of possible existence.

His long last essay is a new one, written for the book. In it he explores modal set theory in depth. Modality no longer figures as an alternative to the reification of abstract objects. By now Parsons is reveling in both.

His variables range over objects in all possible worlds, and his predicates vary in extension from world to world unless they enjoy *rigidity,* whereof two senses emerge. Predicates determine classes as usual, and these are distinguished from sets, but now they come to be disposed in an iterative hierarchy like that of sets. Moreover, the modalities infect the classes with an intensionality that accords them rather the character, Parsons suggests, of attributes.

An assortment of alternative systems is developed. One of them invokes a bracketing device to indicate that the bracketed segment of the formula is to be read as governed by just the first n of the outlying modal operators. Brands of modality vie with one another.

> [E]ither we have to leave the two modalities side by side, or we must seek a third interpretation of modality, more general than both metaphysi-

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> cal modality and our own restricted modality. . . . One's first thought might be that it should be a strictly logical modality . . . I conclude that what we should be looking for is a form of mathematical modality (327).
>
> [W]e must regard the problem of formulating [a perfectly general modal set] theory as unsolved, and we must even leave some doubts concerning what its logical framework should be (331).

This concluding essay, we see, is one that will richly reward those who are bored by clear skies and desert landscapes.

The book contains dozens of misprints, some of which bear notice. Page 31, line 15, read "intuitionism." Page 59, line 4, for "y" read "Y." Page 120, sixth line up, for "$2 =$" read "$2 + 2 =$." Page 122, eleventh line up, and page 133, tenth line up, and page 161, line 19, read "is." Page 213, formula (4), for "(n)" read "(n, s)". Page 246, line 11, for last "that" read "from." Page 310, line 5, for alpha read "a." Pages 315/6, all three formulas, for "a" or "a" read "y" or vice versa. Page 332, line 12, wrong symbol; consult context. Page 351, fourth line up, read "Jean-Louis."

W. V. QUINE

Harvard University

NOTES AND NEWS

The International Berkeley Society will hold a Tercentenary Conference in Newport, R.I., on March 7–10, 1985. The participants will include Robert Adams, Margaret Atherton, Michael Ayers, David Berman, Martha Bolton, Harry Bracken, Genevieve Brykman, Philip Cummins, Willis Doney, Daniel Garber, Desiree Park, George Pitcher, Ian Tipton, Margaret Wilson, and Kenneth Winkler. Further information can be obtained from: International Berkeley Society, Department of Philosophy, Brown University, Providence RI 02912.

The Society for Philosophy and Psychology is calling for contributed papers and symposium proposals for its 11th annual meeting, May 15–17, 1985 in Toronto, Canada. The Society consists of psychologists and philosophers with common interests in the study of behavior, cognition, language, the nervous system, artificial intelligence, emotion, consciousness, and the foundations of psychology. More information may be obtained from Stevan Harnad, Behavioral & Brain Sciences, 20 Nassau Street #240, Princeton, NJ 08540.

The Society for the Philosophy of Sex and Love will be meeting with the Eastern Division of the American Philosophical Association (December 28,

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1985, in Washington, D.C.) and with the Western Division of the APA (April, 1986, location not yet available). Papers to be considered for presentation should reach the address above by May 1, 1985 (Eastern) and by September 15, 1985 (Western). Papers may be on any issue in the domain of sex and love. Further information may be obtained from Alan Soble, Box 13, Philosophy Dept., Moorhead State University, Moorhead, Minn. 56560.

In the election that was held this summer, Diana Meyers of Cornell University was elected to a three-year term as National Executive Secretary of the Society for Philosophy and Public Affairs. Henceforth inquiries, dues, and other SPPA business should be directed to her at the Department of Government, Cornell University, Ithaca, NY 14853. A new national board was elected to serve a two-year term. Its members are: Fred Berger (California/Davis), Christina Hoff (Clark), Douglas Lackey (CUNY). Professor Meyers requests those interested in organizing or participating in the SPPA sessions held during the meetings of the American Philosophical Association write to her.

The Alexander von Humboldt Foundation in the Federal Republic of Germany has made a Senior U.S. Scientist Award to Adolf Grünbaum, Andrew Mellon Professor of Philosophy at the University of Pittsburgh, in recognition of accomplishments in research.

The Center for Dewey Studies, Southern Illinois University at Carbondale, announces the 1984 John Dewey Essay Project awards: first place ($1,000) has gone to Thomas M. Jeannot of St. Louis University for "A Study in Contrasts: Dewey's Naturalism, Rorty's Therapeutic, and Whitehead's Panpsychism"; second place ($500) has gone to James D. Grant of Harvard University for "John Dewey and Ralph Waldo Emerson: An Examination of Continuities in Thought."

The Bertrand Russell Society will award a Doctoral Grant of $1000 to a currently enrolled doctoral candidate in any field whose proposed dissertation best gives promise of dealing in a significant way with the thought, life, or times of Bertrand Russell. The Grant is unrestricted. It might, for instance, be used for travel to the Russell Archives (in Canada), or for typing the dissertation. Further information may be obtained from: Hugh S. Moorhead, Chairman, Philosophy Department, Northeastern Illinois University, 5500 N. St. Louis Avenue, Chicago, Ill. 60625.

Under a grant from the National Science Foundation (EVIST program), Vivian Weil, Senior Research Associate for the Center for the Study of Ethics in the Professions and John Snapper, Assistant Professor of Philosophy and Faculty Associate of the Center, are Principal Investigators for a 1985 Conference on "Ethical Implications of Trade Secrecy, Patents, and Related Property Controls for Science and Technology." Scholars, research managers, scientists, engineers, and others interested in the project may obtain information from either Dr. Weil or Dr. Snapper at the Center for the

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Study of Ethics in the Professions, Illinois Institute of Technology, Chicago 60611.

Bernard Gert reports that his book *The Moral Rules* (New York: Harper & Row, 1970) is temporarily out of print; it is being revised and expanded. Professor Gert hereby gives his permission to anyone to copy all or part of the book for class use during this period, free of charge. He would appreciate receiving any comments or criticisms. Further information can be obtained from Bernard Gert, Department of Philosophy, Dartmouth College, Hanover, N.H. 03755.

The Philosopher's Computer Network invites all who are interested in computers and philosophy to participate in an information-sharing network. Each participant receives a newsletter containing a list of participants and their interests. Advertisements are printed at no charge. More information may be obtained from Michael Swanson, 415 Herondo #334, Hermosa Beach, Calif. 90254.

The Center for Ethics, Medicine, and Public Issues at Baylor College of Medicine is pleased to announce its first annual program of fellowships in ethics and medicine from May 28 to June 21, 1985. This program, funded by a grant from the Exxon Education Foundation, enables college faculty teaching medical ethics to have four weeks of clinical experience with ethical problems in reproductive biology, neonatal intensive care, pediatric hematology, general medicine, experimental therapies, emergency psychiatry, medical intensive care, oncology, and geriatric medicine. Further information and application forms can be obtained from Baruch Brody, Center for Ethics, Baylor College of Medicine, Houston, Texas 77030. Applications are due by March 1, 1985.

The Social Philosophy and Policy Center of Bowling Green State University has appointed John Ahrens (North Carolina/Wilmington) as Assistant Director. John Gray, who is a Fellow of Jesus College, Oxford, has been appointed a Distinguished Research Fellow of the Center and will be spending most summer terms in residence there.

The Philosophy Department of Cabrini College is pleased to announce that the first annual Larry Sedler Memorial Lecture was delivered by Thomas Busch, Chairperson of Philosophy at Villanova University. His talk was entitled "Sartre-Post Existentialism."

There will be a Conference on Critical Thinking at Christopher Newport College April 11-12, 1985. The importance of recent work in critical thinking and informal logic for education and teaching will be stressed. Speakers will include Michael Scriven, Robert H. Ennis, Matthew Lipman, Ralph H. Johnson or J. Anthony Blair, and Philip Pecorino. Workshops will be conducted also. Further information can be obtained from John Hoaglund, Dept. of Philosophy, Christopher Newport College, 50 Shoe Lane, Newport News, Va. 23606.

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The Department of Philosophy at the University of Colorado in Boulder is pleased to announce the appointments of Robert Cummins as Professor of Philosophy and of Nancy Davis as Assistant Professor of Philosophy beginning in Fall 1984. Robert Cummins will also be joining the Institute for Cognitive Science at the University, and Nancy Davis will also be joining the Center for the Study of Values and Social Policy, recently established by the Philosophy Department.

The Institute for Logic and Cognitive Studies, University of Houston/Clear Lake, invites submission of papers for presentation at its second annual conference. The theme of the conference will be "Logic, Logic Machines, and Public Education." It is the intention of the Institute to publish selected papers in biennial proceedings. Proposals for symposia and workshops are also invited; symposium proposals in any area of artificial intelligence, including its history and philosophy, will be considered. Further information may be obtained from Paul A. Wagner, Director, Institute for Logic and Cognitive Studies, University of Houston/Clear Lake, Box 269, Houston, Texas 77058.

The Department of Philosophy at the University of Maryland, College Park, announces the appointment of Michael Slote (Trinity College, Dublin) as Chairman of the Department, effective January 1, 1985. Jonathan Glover (New College, Oxford) will be Visiting Professor during the Spring semester, and Alberto Cordero (Universidad Cayetano Heredia, Peru) will be Visiting Associate Professor for the year. John Bacon (University of Sydney) will be Visiting Professor for the year, while Stephen Stich of the Maryland Department is Visiting Professor at Sydney.

The Department of Philosophy at the University of Oklahoma announces the appointment of John Biro as chairman. It is also pleased to announce that Brent Mundy joined the department as an Assistant Professor in the fall of 1983 and that Cindy Stern and James Kostman have been appointed Assistant Professors beginning in the fall of 1984.

The Department of Philosophy of the University of Rochester announces a call for papers for the Department's third annual conference, "Aristotle's Philosophy of Mind and Modern Cognition", April 5 and 6, 1985. The conference topic is the relation of Aristotle's work to recent developments in the philosophy of mind. Inquiries should be directed, and papers submitted no later than February 1, to Earl Conee, Department of Philosophy, University of Rochester, New York 14627.

The Department of Philosophy of Tulane University announces a Conference on Positive and Negative Duties for March 29/30, 1985. Featured speakers will be James Fishkin and Jan Narveson. Additional papers on any theoretical or applied aspect of this topic are solicited for consideration, with a deadline of January 15, 1985. Inquiries and submissions should be sent to Eric Mack, Newcomb Hall, Tulane University, New Orleans, La. 70118.

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The trustees and editors of the *Journal of Philosophy* join the members of the department of philosophy of Columbia University and of the department of philosophy at the Claremont Colleges in mourning the death of

HERBERT WALLACE SCHNEIDER, 1892–1984

editor, teacher, author, internationalist, friend.

Professor Schneider's first article in the *Journal,* in March of 1917, initiated an exchange with Wilbur N. Urban on the Theory of Value. In July of 1924, he joined Frederick J. E. Woodbridge and Wendell T. Bush as editor of the *Journal,* and was influential in setting the direction of the *Journal*'s development; he was an editor from 1924 to 1962 and consulting editor thereafter. He taught at Columbia from 1917 until 1957, served as chairman of the philosophy department from 1939 to 1945, and was president of the Eastern Division of The American Philosophical Association in 1948. From 1953 to 1956, on leave from Columbia, Professor Schneider lived in Paris for three years as head of the UNESCO division of International Cultural Cooperation. After his "retirement" from Columbia, as professor emeritus, he moved to Claremont, California; he was active in the formation of the graduate program of the United Colleges of Claremont, for some years director of the Blaisdell Institute for Advanced Study in World Culture and Religions, one of the founders of the *Journal of the History of Philosophy* in 1963, teacher and administrator at Claremont until a few years before his death.

Professor Schneider's writings were many and wide-ranging, including more than a score of books and more than a hundred articles and book reviews. They include *Making the Fascist State* (1928) and *The Fascist Government of Italy* (1936); *The Puritan Mind* (1930), *Religion as a University Concern* (1931), and *Religion in 20th Century America* (1952); *A History of American Philosophy* (1946) and *Sources of Contemporary Philosophical Realism in America* (1964); and *Ways of Being* (1962), expanded from the Woodbridge lectures he delivered in 1961 at Columbia.

As editor, author, teacher, and administrator during his long career, Professor Schneider served the philosophical community and the wider international cultural community with dedication and distinction.

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Human, All Too Human

A Book for Free Spirits

By Friedrich Nietzsche
Translated by Marion Faber,
with Stephen Lehmann
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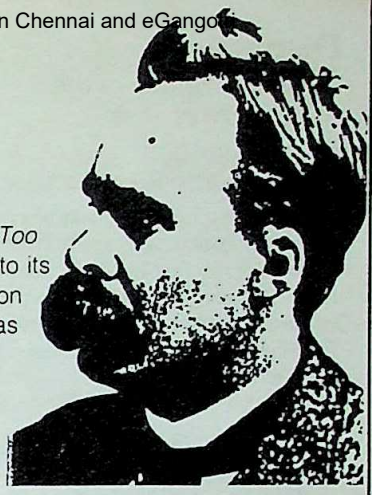

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